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Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers and Communications in the present Volume are, Mr. Charles Sylvester; J. S. Traill, M. D.; Mr. Richard Winter; Philommatos; A Chemist; J. G. C.; William Henry, M. D.; Thomas Thomson, M. D. F. R. S. E.; Mr. William Skrimshire, Jun.; John Gough Esq.; G. S. Gibbes, M. D.; N. R. D.; Joseph Reade, M. D.; Mr. Donovan; Mr. Barraud; Dytiscus; J. Garnett, Esq.; The Rev. T. Vince, Professor of Astronomy at Cambridge; and Frederick Stromeyer, M. D. Professor at Gottingen.

Of Foreign Works, M. La Lande; J. Carradori de Prato, M. D.; M. Decandolle; M. Trommsdorff; M. Bernhardi; M. Robiquet; M. Chevreul; M. C. A. Cadet; M. Vauquelin; M. Boullay; M. Hisinger; M. Berzelius; M. Thenard; Dr. Berger; Mr. Adolphus Ledhuy.

And of British Memoirs abridged or extracted, Mr. Davis; Humphry Davy, Esq. F. R. S. M. R. I. A.; William Hasledine Pepys, Esq.; Right Hon. Sir Joseph Banks, Bart, K. B. P. R. S. &c.; Thomas Andrew Knight, Esq. F. R. S.; Everard Home, Esq. F. R. S; William Herschell, L. L. D. F. R. S.; William Allen, Esq. F. L. S.; John Jerome Schroeter, F. R. S.; Mr. Lewin Tugwell; Dr. Cogan; Mr. Robert Hallett; Brigade Major William Lambton; C. H. Parry, M. D.; T. Davis, Esq.; W. Matthews, Esq.; Dr. Anderson; Mr. Edward Martin; W. H. Wollaston, M. D.; and W. Roxburge, M. D.

The Engravings consist of 1. Guide to the Constellations; 2. Mr. Davis's Machine for Glaziers; 3. Mr. Davy's Experiments on Galvanism; 4. New Eudiometer by W. H. Pepys, Esq.; 5. Experiments on inflected Light, by Mr. R. Winter; 6 and 7. Dr. Hershell on coloured concentric Rings; 8. Dr. Joseph Reade's Calorimeter; 9. Messrs. Allen and Pepys on Carbonic Acid, and the Diamond; 10. Mr. Barraud's Mercurial Pendulum; 11. Dr. Hershell on the Planet Vesta; 12. Mr. Tugwell's new Method of Roofing Houses; 13. Representation of a Mineral Bason in South Wales.

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ARTICLE I.

An Account of the relative Situations of the different Stars, by which the principal Constellations may be distinguished. From LA LANDE'S *Astronomy, third Edition, Art.* 743, *&c**.

THE great Bear is a constellation, which is always visible, it is easily known from the seven stars of which it consists (see Pl. I, Fig. 1). Four of them are in the body and three in the tail; and the two farthest from the tail α and β are called the pointers, because a line drawn from β to α, if produced, will pass on to the pole star, which is about as far from α as α is from η. The convex side of the tail is turned toward the pole.

The constellations visible at all times.

Ursa Major.

Cassiopeia is opposite to the great Bear, the polar star lying between them, so that if a line be drawn from ε Ursæ Majoris

Cassiopeia.

* The following paper is a free translation of all that part of Mr. la Lande's work, which can be of most service to those, who have not the advantage of any astronomical instruments, by which they may measure angles, or take observations on the meridian. At the same time, however, that I endeavoured to render the meaning as precisely as I could, I thought myself at liberty to make any small alteration, which would more clearly point out the sense of the passage, or adapt it to the use of the English reader.

N. R. D.

Majoris through the pole star, it will pass through the middle of Cassiopeia. This constellation consists of six or seven stars, which form a Y, or, as some describe it, a chair turned on its back. This description is by no means distinct, but there is little danger of any mistake; because several of these stars are of the second magnitude (Fig. 2).

Ursa Minor. The little Bear is nearly of the same form as the great Bear, but the figures though parallel are reversed with respect to one another. The pole star is of the 3d magnitude at the extremity of the tail, the four next stars to it are only of the 4th magnitude; but the two last, which make up the square, are of the 3d, and are called the Guards. These last are in a line drawn through the centre of the great Bear, perpendicular to its longest side.

Arcturus. Arcturus, a star of the first magnitude in Bootes, is distant 31° from the tail of the great Bear; and if a line be drawn through ζ and η, the two stars at the extremity of the tail, it will point to Arcturus.

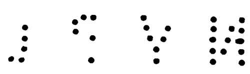
Lyra and Capella. When the great Bear is on the meridian, Lyra and Capella, two stars of the first magnitude, are seen, one in the east, the other in the west, in a line drawn through the pole star, perpendicularly to that which joins the great Bear and Cassiopeia. Capella is to the east when the great Bear is under the pole; and then, if their altitude is the same, it is almost equal to that of the pole star.

Draco. Draco is on the line drawn from α Ursæ Majoris through the Guards of the little Bear, between which and Lyra may be observed the four stars in the shape of a lozenge, which form the head; the tail lies between the pole star and the body of the great Bear. The line through the Guards points to η Draconis, which is north of θ and south of ζ in the line, which is directed towards the pole of the ecliptic.

Cepheus. This line produced a little farther towards δ and ϵ Draconis will pass between β and α Cephei.

Cygnus. The line drawn from the pole star to these two last mentioned stars in Cepheus will pass near to the tail of the Swan, which is a beautiful object, and never sinks below the horizon of London.

The constella- Having now gone through those constellations, which are always



always above our horizon, we will next proceed to those, which are visible in a winter's evening. *tions visible in a winter's evening.*

About 7 or 8 o'clock P. M. in the months of January and February, Orion is visible in the south. It consists of seven stars, four of which are at considerable distances from each other, and in the centre of them are three others of the 2d magnitude, which are much closer and in a straight line. This is a very remarkable constellation and may be easily recognised if compared with Fig. 3. *Orion.*

The three bright stars in the belt of Orion point on one side to the Pleiades and on the other to Sirius. Sirius is the brightest of all the fixed stars, and is remarkable for its radiancy and brilliance: it lies on the south-east of Orion. The Pleiades are on the north-west of Orion, and form a group of small stars, which may be easily distinguished, as they lie a little above the line drawn through the three stars of the belt of Orion. *Pleiades and Sirius.*

Aldebaran, or the Bull's eye, is a star of the first magnitude very near the Pleiades, and situated between them and γ the star in the western shoulder of Orion. *Aldebaran.*

Procyon or Canis Minor is a star of the first magnitude, situated to the north of Sirius and the east of Orion: it makes nearly an equilateral triangle with Sirius and the belt of Orion. *Procyon.*

The Twins are two stars of the second magnitude, situated about midway between Orion and the great Bear. They may also be distinguished by drawing a line from Rigel (which is β or that of the four outermost stars in Orion, which lies in the south-west) through ζ the eastern star in the belt; since this will direct us to the heads of the Twins: and again if we draw a line from ζ or ϵ of Orion to δ and β of the great Bear, it will pass over one of the paws of the Bear, and also by the heads of the Twins. This same line will cross the feet of the Twins, and will pass very near α, the star in the eastern shoulder of Orion. The feet of the Twins are marked by four stars in a straight line perpendicular to the direction here given. *Gemini.* *Rigel.*

The line drawn from Rigel through γ in the western shoulder of Orion, will pass on the north through ζ a star of the third magnitude, on the southern horn of Taurus: it *Taurus.*

is about 14° from γ Orionis, or the same distance at which γ Orionis is from Rigel. β, the northern horn of the Bull, is also called the foot of Auriga, it is of the second magnitude, and in the line drawn from α in the eastern shoulder of Orion through ζ Tauri, the southern horn. The ecliptic passes between the two horns.

Leo. The Lion may be recognised by the same stars α and β in the great Bear, which serve to point out the polar star. They are distant about 45° north of the Lion, which forms a large trapezium, in which there is a star of the first magnitude called Regulus, or Cor Leonis; it is in a line with Rigel and Procyon, but at the distance of 37° from the latter. β also, a star of the second magnitude in the Lion's tail, is a little on the south of a line drawn from Arcturus to Regulus: it is 24° to the east of Regulus, and makes an equilateral triangle with Spica Virginis and Arcturus.

Regulus, or Cor Leonis.

Cancer. Cancer is a constellation of small stars, which are distinguished with difficulty. The nebulous star in Cancer is less perceptible than the Pleiades, and we meet it nearly half way between the centre of Gemini and the Cor Leonis, or in the line which joins Procyon and the tail of the great Bear. From ε, the middle star of the belt of Orion, there proceeds a train, which is called the Sword; it contains the Nebula. A line drawn through the Sword and the star ε points towards ζ, the southern horn of the Bull, and beyond it to the middle of Auriga.

The Nebulous stars in Cancer and Orion.

Auriga. Auriga forms an irregular pentagon, the most northern star of which is Capella: it is of the first magnitude, and may be found by drawing a line through δ and α, the two most northern stars in the body of the great Bear.

Capella.

Aries. Aries, the first of the twelve constellations in the Zodiac, consists principally of two stars of the first magnitude, situated near one another: β, the more western of the two, is accompanied by γ, of the 4th magnitude, which has been called the first star in Aries, because it was once the nearest star to the equinoctial point. This constellation is in the same line with Aldebaran and Procyon, from the former of which it is distant about 35°.

Perseus. The belt of Perseus consists of three stars, one of which is of the second magnitude. They form a curve with its convex

convex side turned towards the great Bear. It might be sufficient to mention, that they lie in the line drawn from the pole star to the Pleiades; but they may also be found by producing a line through Gemini and Capella. The line drawn from the belt of Orion through Aldebaran passes through β, the head of Medusa, which Perseus holds in his hand: this star, which is also called Algol, is changeable.

The Swan is a very remarkable constellation: it forms a large cross, and contains a star of the second magnitude. A line drawn from Gemini through the polar star will meet the Swan at about an equal distance on the opposite side: at some seasons of the year they are both at the same time above the horizon. But we shall have another means of distinguishing this constellation, when we are acquainted with that of Pegasus. Cygnus.

The square of Pegasus is formed by four stars of the second magnitude: the most northern is the head of Andromeda. The line drawn from α and β of the great Bear through the pole star will pass across the middle of these four stars. A line drawn from the belt of Orion through Aries will lead to the head of Andromeda: one drawn from the Pleiades through Aries will lead to γ in the wing of Pegasus: the other two stars are to the west; the northern is β and the southern α. Pegasus.

The diagonal drawn through γ and β passes on northwest towards α in the tail of the Swan: the other diagonal, drawn through α and the head of Andromeda, points north-east to the belt of Perseus, having first passed β in the girdle, and γ near the foot of Andromeda: these two stars (β and γ) are of the second magnitude, and divide the space between the head of Andromeda and the belt of Perseus into three equal parts. The line which connects them is at right angles to that which would join Aries and Cassiopeia.

The constellations visible in a summer's evening do not possess such strongly distinguishing characters as those, which we have just been describing: but a person who has made himself acquainted with those, which may be seen in winter, The constellations visible in a summer's evening.

winter, will find that the knowledge of them will assist him very much in ascertaining the rest.

Spica Virginis. The middle star (ζ) in the tail of the great Bear is on the meridian over the pole star, about 9 o'clock in the latter end of May. Spica Virginis, a star of the first magnitude, will then appear on the meridian in the south at the altitude of about 28° 30′. The diagonal drawn through α and γ in the great Bear will nearly pass through this star, although at the distance of 68°. Moreover Spica Virginis makes nearly an equilateral triangle with Arcturus and the Lion's tail, from which it is distant about 35°.

Corvus. At this time also the four principal stars in the Crow are a little to the right, below Spica Virginis. They form a trapezium, and are situated in the same line with Lyra and Spica Virginis.

Hydra. If from δ and γ, the last stars in the square of the great Bear, a line be drawn through Regulus, it will meet, at the distance of 22° to the south, the star called Cor Hydræ. The head of the Hydra is to the south of Cancer, between Procyon and Regulus; but it is a little south of the line which joins them. This constellation extends from Canis Minor to the part of the heavens, which is situated below Spica Virginis and part of Libra. Between it and the Crow

Crater. is the Cup.

Lyra. Lyra, a star of the first magnitude, is one of the most brilliant in the whole heavens. The situation with respect to Arcturus and the pole star is such, as to make nearly a right angle to the east in Lyra.

Corona Borealis. The Northern Crown is a small constellation, situated between Arcturus and Lyra: it is near Arcturus, and may be easily distinguished by the seven stars, of which it is composed; they are arranged in a semicircular form, and one of them (α) is of the second magnitude. ζ and η, the two last stars in the tail of the great Bear, are in a line with the Crown.

Aquila. The Eagle contains a very bright star of the second magnitude, which is in the south of the Lyre and the Swan. It is easily distinguished, because it is situated between β and γ, two stars of the third magnitude, which are very close and form a straight line with it.

The

The great circle, which passes through Regulus and Spica Virginis, is nearly the same with the ecliptic, and if it be produced to the eastward, it meets the Scorpion, a remarkable constellation on account of the four stars in its head, which form a large arch from N. to S. round Antares, or the Cor Scorpionis, which is placed as a centre to them. One of the four stars is of the 2d magnitude, and Antares is a bright star of the first magnitude. Scorpio. Antares.

The Balance contains two stars of the second magnitude, which form the two scales: the line which connects them is nearly perpendicular to that which may be drawn from Arcturus to Antares, and they lie a little to the south of the middle of this line of direction. The southern scale is situated between Spica Virginis and Antares, and these three stars are all very nearly in the ecliptic. Spica Virginis is at the distance of 21°, and Antares at the distance of 25°, from the southern scale. Libra.

Sagittarius is a constellation, which follows the Scorpion, being a little to the east of it. It is in the line, which, passing through Spica Virginis and Antares, follows nearly the direction of the ecliptic: it contains several stars of the third magnitude, which form a large trapezium, two stars of which, together with two others, form a second trapezium, perpendicular as it were to the first. Sagittarius may be known by a line drawn through the middle of the Swan and Eagle; for it is 35° south of the Eagle, or nearly the same distance from it as the Eagle is from the Swan. Sagittarius may also be known by the diagonal drawn from the head of Andromeda to α Pegasi, the same line, which produced towards the north points out the belt of Perseus. Sagittarius.

The line drawn from Antares to the pole star passes through Ophiucus, or Serpentarius, and then through Hercules. It is rather difficult to know these constellations, and therefore they must be described more particularly. The line drawn from Antares to the Lyre passes near the head of Ophiucus, which is not far from that of Hercules, and lies to the south-east of it. They are marked by two stars of the second magnitude, and the line which connects them points to the Crown; it also passes through γ Herculis at the distance of 13° from the head of Hercules. β Herculis is at the distance of Ophiucus and Hercules.

Ophiucus and Hercules.

of 3° to the north-east of γ, the line drawn from it to γ points on the north to ϵ Herculis, and on the south, or rather south-west, to α Serpentis, which forms nearly an equilateral triangle with the head of Hercules and the Crown. The line drawn from the head of Ophiucus to the southern scale of the Balance passes through δ and ϵ Ophiuci, two stars of the third and fourth magnitudes, which are at the distance of only 1° 20′ from one another, and in the line drawn perpendicular to that which was last described. δ lies to the north-west of ϵ, and these two stars point on the south-east towards ζ in the western knee of Ophiucus, which is 7½° from ϵ. This same direction will lead near to η, the star in the other knee of Ophiucus, which is about 9½° to the south east of ζ. These same stars δ and ϵ point a little below α Serpentis, and, if considered as one group, they would make nearly an equilateral triangle with α Serpentis and β in the northern scale, 4½° north-west of α Serpentis is δ, and 3° south-east is ϵ of the same constellation. The direction of these three stars is also towards δ and ϵ Ophiuci, which are 11° from ϵ Serpentis. β and γ, the two stars in the eastern shoulder of Ophiucus, are in the line drawn from the head of Hercules to the head of Sagittarius: this line passes a little to the south-east of the head of Ophiucus. β is 8° and γ 11° from the head of Ophiucus. A line drawn through them would pass between the two heads of Hercules and Ophiucus: the line connecting these two heads points to θ at the extremity of the tail of the Serpent, which is 22° east of the head of Ophiucus. The line drawn from the most eastern stars in the Crown (which are on the side turned towards the Lyre) to α Serpentis passes by the head of the Serpent, between γ and β, two stars of the third magnitude: β is the more western of the two. The western foot of Ophiucus lies between Antares and β, the northern star in the head of the Scorpion: the eastern leg is between Antares and μ Sagittarii, which is the highest and most western star in the bow.

Capricorn.

Capricorn may be found by producing the line drawn from the Lyre to the Eagle: this line will pass through α and β two stars of the third magnitude in the head of Capricorn. These stars are only 2° from one another. Farther

to

to the east by 20° are two other stars, γ and δ, situated east and west about 2° asunder: they are in the tail of Capricorn.

Fomalhaut, or the mouth of the southern Fish, is a star of the first magnitude, and is pointed out by a line drawn from Aquila to the tail of Capricorn. Fomalhaut lies about 20° to the south east of δ Capricorni. Fomalhaut.

The Dolphin is a small constellation situated about 15° east of the Eagle. It consists of a lozenge of four stars of the 3d magnitude. A line drawn from the Dolphin perpendicularly through the middle of γ, α, and β, the three stars in the Eagle, will pass through θ in the extremity of the tail of the Serpent. Delphinus.

Aquarius is found by a line drawn from the Lyre through the Dolphin, and carried on about 30°, which is as far beyond the Dolphin as the Dolphin is distant from the Lyre: Aquarius lies a little to the east of this line. A line drawn from the Dolphin to Fomalhaut will pass entirely across the constellation of Aquarius, and it will pass about midway between α and β, two stars of the 3d magnitude, in the shoulders of Aquarius. They are the most remarkable stars in the whole constellation, and are about 10° distant from one another. Aquarius.

The Whale is a large constellation, situated on the south of Aries, and extending through a space, which is equal in length to the distance of the Pleiades from the four stars in Pegasus. A line drawn from the girdle of Andromeda, and passing between the two stars in Aries, will meet α, a star in the mouth of the Whale, which is of the 3d magnitude, and 25° from the horns of Aries. A line drawn from Capella through the Pleiades will also pass south of α Ceti. A line drawn from Aldebaran through the mouth of the Whale will pass through β, a star of the 2d magnitude in the tail: β is 42° west of α, and very near the constellation of Aquarius. The square in Pegasus is alone sufficient to point out the Whale; for the line drawn through the two most southern of these stars passes between Aries and the knot of the Fishes, and will meet the head of the Whale: and the line drawn through the most eastern stars in the same square points to the tail. Between the head and the tail are situated γ and δ, and between δ and the tail is o, a changeable Cetus.

star,

possessed by that metal, though I could fully speak to the facility with which it could be worked into vessels, and of its application to other purposes. I was still, for want of longer experience, not decided as to its changeability when exposed to the action of water and air.

Zinc might be supposed easily oxidable.

From the great affinity which zinc possesses for oxigen, it might be expected to oxidate with great avidity, and on that account be rendered useless in the situations above alluded to; but, to the astonishment of most theorists, the contrary proves to be the case. Many specimens of zinc, both in the state of sheets and wire, have been exposed in the open air, as well as in damp rooms, without undergoing any other change than that of colour. Indeed it appears, that a piece of polished zinc will lose its lustre, and assume a blue gray colour, when exposed in a damp room for the space of a few weeks. An oxide is formed upon the surface, which, though of an imperceptible thickness, is so exceedingly hard, and at the same time so insoluble, that it resists all the future attacks both of the air and of water. From numerous experiments I have ascertained, that copper is much more liable to waste than zinc in sea water, and even in strong solutions of muriate of soda. There cannot be therefore a doubt of its ready application to the sheathing of ships.

But it is not, except superficially.

Less affected than copper by seawater.

Its superiority over lead or copper for various purposes.

For the purposes of roofing houses, forming cisterns, pumps, pipes, &c., it possesses many advantages over lead and copper. In the first place it is equally durable with those metals, without possessing any of their deleterious effects. It is also capable of being lapped and soldered with the same facility as sheets of copper, lead, or tinned iron plates; and may be worked to advantage equally by the brazier, the plumber, and the tinman. Its little specific gravity, which is to that of lead as 7 to 11, compared with its greater strength, which is 15 times that of lead, gives it a decided advantage over that metal in point of price. Allowing the sheets of zinc to be only ⅕th the thickness of lead, the zinc will come in at one third the price of that metal. Its advantage in a similar point of view over copper will not admit of a question.

General size of the sheets.

The sheets are generally made 2 feet by 4, and can be rolled as thin as 6 ounces to the square foot.

Sheets

Sheets or wire of zinc may be purchased of Mr. Philip George of Bristol, or of Messrs. Harvy and Golden, 98, Houndsditch, London: Of whom may also be had, vessels and utensils of any form. They likewise undertake the roofing of houses, or sheathing vessels, with zinc.

By giving the above a place in your much esteemed Journal, you will much oblige

Your obedient servant,

CHARLES SYLVESTER.

P. S. I observed some time ago in your Journal, experiments by Mr. Davy on the subject of the production of the muriatic acid and fixed alkali by galvanism, in which some of my former experiments were alluded to. I do not think Mr. Davy is decisive on the subject, and have not a doubt of very soon confirming all that I have previously asserted.

Sheffield, Nov. 20, 1807.

III.

Description of Mr. Davis's *improved Machine for Painters and Glaziers**.

THE frequent accidents which happen to painters and glaziers, from the unsteadiness of their machines, and the consequent misery brought upon their families, stimulated Mr. Joseph Davis, of the Crescent, Kingsland Road, to endeavour at their improvement. The result was the machine delineated in plate I, which may be made perfectly firm and secure, without occasioning any injury to the wainscoting or paint. In those cases however, where the bottoms of the windows are flush with the floor, as is usual in the best apartments of modern houses, neither the common machine, nor this with the improvement intended for general use, can be applied: but Mr. Davis has contrived an additional piece to be used on such occasions, which renders it equally secure. Machine for preventing accidents to painters and glaziers.

* From the Trans. of the Society of Arts for 1806, p. 138.

Fig.

Description of the painter's and glazier's machine.

Fig. 4, plate I, Represents the machine: the part *a* is similar to that used by glaziers, which is placed on the outside of the window. *b*, is an additional moving piece, which presses against the inside of the window frame, and is brought nearer to, or removed farther from it, by means of the male screw *c*, and its handle *d*.

Fig. 5, Shows the lower part of a window, and the manner in which the moving piece *b*, including a female screw, acts against the inside of the window frame.

Fig. 6, Shows a cross bar introduced in place of the moving piece last mentioned, which bar extends from one window side to the other, and explains how the machine may be used, where any injury might arise from screwing the moving piece in the centre of the recess of the window.

The general improvement consists in the use of a screw on that end of the frame which is within the house, and which keeps the machine steady and firm, instead of the two upright irons, which are put through holes made in the top plank of the machine, in the common mode, and which occasion the machine to be very unsteady in use, and liable to accident. There are two blocks marked *d*, *d*, in Fig. 4, which may be occasionally put in, or taken out, according as the stone work under the window may require.

IV.

Answer to some Observations of Mr. Dispan on the pretended Attraction of Surface between Oil and Water: by J. CARRADORI DE PRATO, *M. D**.

Oil spreads on water from an attempt to preserve their level between two fluids of different gravities.

MR. Dispan, a celebrated professor of chemistry, imagines that the phenomenon of the spreading of oil on the surface of water arises simply from an effort of libration between two bodies of very different specific gravities, as oil and water are. "A drop of oil," says he, "falling on still water, is a sphere composed of extremely movable particles, disposed by its difference of gravity to yield the level to the water, and

* Annales de Chimie, vol. LXII, p. 65, April, 1807.

consequently

Guide to the Constellations.

Mr. Davis's Machine for Glaziers.
Fig. 4.

consequently to apply itself to the whole surface in a very thin stratum. At the instant of its fall the drop of oil displaces a volume of water equal to its momentum. But presently, as the fluidity of the oil gives its particles the faculty of gliding one upon another, the reaction of the water having raised the drop, its particles, finding no obstacle, slide down on all sides with rapidity, till the whole is reduced to a very thin stratum. Thus in this fact there is nothing, that justifies the pretended affinity of surface between oil and water: on the contrary, instead of an application or reunion of surface, there is rather a division and a separation; since the drop of oil, which is spread upon the water, divides itself into an infinite number of others."

This opposed by facts.

With all the respect due to the talents of the professor, it appears to me, that his explanation is strongly contradicted by the facts on which mine is founded. I have adduced several experiments, in two papers inserted in the Transactions of the Italian Society of Sciences, Vols. XI and XII, which show, that there is a *physical force*, by which the spreading of oil on water, and on other fluids, is determined. With these professor Dispan could not be acquainted, otherwise he would have refrained from giving his explanation of this phenomenon.

It is not necessary that the drop should fall, so as to cause reaction.

I would ask Mr. Dispan, how he accounts for the spreading of oil on water, when the drop is not let fall upon it, but cautiously applied, without making the least impression upon the water, so that no reaction is produced, that can overcome the affinity of aggregation of the oil. A drop so applied certainly spreads completely, and particularly if the water expose an extensive surface. It will be still more difficult for him to explain, how a drop of the milky juice of spurge, applied on water in the same manner, spreads over it in the twinkling of an eye, and covers it with a very thin pellicle; or even why a small quantity of wheat flour, or any other fecula, thrown on water, instead of falling to the bottom, spreads on its surface. There is no libration, no effort, no reaction of the water here. It appears to me, that these facts are much better explained by the principle I have established

Juice of spurge spreads like oil.

Farinaceous powders to the same.

blished in the two papers abovementioned, than by the mechanical operation imagined by Mr. Dispan.

The oil collecting again into drops no argument.

I know well, that the drop of oil breaks, after it has spread upon the water, and that it collects into other very small drops: but this does not hinder the spreading of the oil from being occasioned by a force, which has compelled it to diffuse itself over the surface, and before acted for a moment.

That a subsequent drop does not spread, not owing to the resistance of the former, since it will not on a part free from oil.

But it is not true, that, if, after the spreading of the first drop, a second and a third be applied to the surface of the water, they do not spread like the first, because they find an obstacle to their movement, and to their gliding on the water, in the fragments of the drop of oil, which was before spread, and occupied its surface: for the phenomenon does not take place, though the drops be applied far from the space occupied by the diffusion of the first drop; and though the eye, assisted by a lens, have previously ascertained, that the surface of the water is free from every obstacle in the place to which the drop of oil is afterward applied.

Oil no obstacle to the spread of the juice of spurge, which impels it in a globule to one side;

I could prove to him likewise, that these obstacles are not sufficient to prevent the diffusion of fluids and other substances, that spread on water. Let a drop of oil spread over the surface of some water in a goblet, and when it has completely covered it, apply a drop of the juice of spurge; this fluid will diffuse itself over the surface with astonishing rapidity, though the surface is already occupied by the oil spread on it before, and, displacing the oil, will force it to accumulate in one or two drops against the side of the glass. It will be just the same, if, instead of the juice of spurge, a little flour be applied to the water; for this will equally spread over the surface of the water, and the oil will be obliged to unite into a single drop or globule, which will sink under the flour, that occupies the surface. What other reason for these phenomena can there be, than the force of adhesion between the surface of the water and these substances?

or of flour, which collects the oil in a globule under it.

Adhesion has some properties in common with chemical affinity.

Lastly I have to observe, that I never asserted there was any thing of chemical action in this phenomenon. I only said in my first paper on the attraction of surfaces, Vol. XI of the Italian Transactions, that the force which natural philosophers

sophers have distinguished by the name of adhesion has some properties in common with chemical attraction, such as the point of saturation, and elective affinity, and that hence it appeared to me, to be very properly termed attraction of surfaces.

I shall not hesitate however, to renounce the principle I have adopted, that of an attraction of surfaces, and retract the explanations of some interesting phenomena, which I have deduced from this principle, and given in the papers already mentioned, if convincing facts and just reasonings show me their incompetency.

V.

Abstract of an Essay on the Medicinal Properties of Plants compared with their external Form and natural Classifications: by Mr. DECANDOLLE*.

NO branch of study deserves the name of science, till it is sufficiently advanced, to be able to determine facts a priori. The materia medica, which rests entirely on the basis of experience, has but three means of forming a judgment of the properties of substances; which are, their sensible qualities, their chemical composition, and their natural analogy. The object of Mr. Decandolle's work, which forms a quarto volume, is to ascertain how far the analogy of the forms of vegetables affords indications of their properties. Indication of the virtues of drugs.

Camerarius first decidedly took up the affirmative side of this question in 1699. He was followed by Isenflamm, Wilke, Gmelin, and more especially by Linneus and de Jussieu. On the other hand Vogel, Plaz, and Gleditsch wrote against it. Notwithstanding what has been written by these learned men, Mr. Decandolle has contrived to treat the question with some novelty, not only in consequence of the progress, that the study of natural affinities has made within these twenty or thirty years; but because, confining himself exclusively to no system, he has formed his deductions not from Have plants similar in appearance similar virtues?

* Annales de Chimie, Vol. LXI, p. 84, January, 1807.

a few solitary families, but from all that compose the vegetable kingdom.

Arguments for it.

He commences with establishing the general proofs, that the medicinal properties of plants are analogous to their external forms. In fact no one will question, that the properties of medicines depend on their physical constitution or chemical composition: but in organized bodies the nature of a production is determined by the form of the organs, since the same aliments, digested by different beings, afford different results; consequently the productions bear some relation to the forms. This reasoning is applicable to the vegetable kingdom, though its classification is not derived from the organs of nutrition, but from those of reproduction; for the natural classes deduced from one function necessarily agree with those deduced from another function.

Observations tending to confirm it.

These general inferences are confirmed by the observation, that herbivorous animals frequently avoid or seek all the plants of the same family: that those, which seem determined to feed only on a single plant, frequently submit to eat plants of the same genus, or of the same family: and that parasitic plants, particularly funguses, display the same preference for certain genera, or certain families. To this may be added, that several foreign drugs, which were formerly supposed to be the production of a single plant, have been found on inquiry to be furnished by several species of the same genus; and that with respect to indigenous simples it is no new thing, for species of the same genus to be substituted for each other. And we may observe, the narratives of travellers inform us, that plants of the same family are often employed for similar purposes in countries remote from each other.

Yet many exceptions.

Notwithstanding these assertions however, which the author supports by several examples, it cannot be denied, that vegetables very closely allied present very striking anomalies. In order to estimate the real weight of these, the author takes a review of the rules of comparison between forms and properties, and this is the part of his work that displays most novelty.

Resemblance in some families of plants

1. In the first place he observes, that, though we arrange species under genera, genera under families, and families under

der classes, in a uniform manner, the groups are far from being separated from each other in an equal degree Thus in certain families plants differ from each other by slight modifications only, while in others they are distinguished by more important characteristics. The analogy between their properties may be presumed to be proportional to the analogy of their structure. closer than others.

2. Secondly, it is contrary to the spirit of the method, to compare the properties of a given organ, or a given juice, with those of another organ, or another juice. This however is one of the most frequent causes, that have led to mistakes on the question. In this discussion the author introduces by the way some new views respecting the structure of bulbs, the body of which he proves to be in reality an abortive stalk, and not a root. Similar organs only and similar juices should be compared.

3. The circumstances of the age of a plant, the season in which it is gathered, the soil in which it has grown, and the degree of light to which it has been exposed, are so many causes of errour, that are to be avoided in the comparison. Adventitious circumstances should be similar.

4. Unequal mixtures or unequal combinations of different principles are found in the organs or juices of certain families; and in these families several of the most apparent anomalies occur. Principles sometimes unequally mixed or combined.

5. In the comparison of properties, we should pay attention to the difference that may exist in the mode of extracting and preparing a drug; for these circumstances frequently have as much influence on their properties as their intrinsic nature. Modes of preparation alter properties.

6. We should exclude from the comparion the mechanical or accidental properties, that arise from circumstances independent of the true nature of substances. Accidental qualities must be excluded.

7. Above all we should most scrupulously attend not to the result of the application of a medicine, but to its mode of acting; for medicines similar in reality act differently according to the organ to which they are applied, or the case in which they are administered, and the contrary. Not the particular result but general mode of action to be regarded.

After laying down these principles the author takes a view of all the families, that compose the vegetable kingdom; and details the properties of all the plants that belong to them,

 including

Mr D's work a complete view of the properties of vegetables.

including not only those that are admitted into our European Pharmacopœias, but those that are employed medicinally by the inhabitants of any part of the globe. In this respect Mr. Decandolle's work gives a complete and methodical display

Of 76 families analogy little violated in 46, and not at all in 23.

of the properties of vegetables: and the result of this exhibition is, that, of seventy-six families, the properties of which are known, there are thirty-seven where the law of analogy is violated, twenty-three where it is completely preserved, and forty-six in which it is observable with a small number of exceptions.

VI.

Analysis of the Siderite, or Lazulite; by Messrs. TROMMSDORFF *and* BERNHARDI*.

Lazulite of Stiria.

THE lazulite was found at first near Waldbach, in Stiria, and afterward in the environs of Wienerisch-Neustadt. It is sufficiently known from the works of various mineralogists. Some time after a mineral was discovered in the country of Salzburg, which was called mollite; but baron Moll has given it the name of siderite, on account of its acknowledged identity with this fossil according to the researches of Mr. Mohs.

Analysed by Klaproth,

Though Klaproth found in the lazulite of Vorau silex, alumine, and iron, he could not ascertain their proportions, from the smallness of the quantity he had to examine. An

and by Heim.

analysis of the siderite by Heim gave 0·65 alumine, and 0·30 iron.

Little analogous to feldtspar.

It is strange, that Messrs. Klaproth, Estner, and Mohs, should fancy there was a great analogy between the lazulite and feldtspar, as analysis shows this analogy to be very slight; and that between their crystallizations and contexture is equally so.

Its usual form.

The most usual form of the lazulite is a regular octaedron with truncated edges, passing to the regular rhomboidal

* Annales de Chimie, Vol. LXII, p. 43, April, 1807. Abridged from Gohlen's Journal by Mr. Vogel.

dodecaedron

dodecaedron. The faces of the octaedron make an angle of 109° 28′ 16″: those of the dodecaedron an angle of 120°: and the former cut these at an angle of 144° 44′ 8″. Beside these several smaller faces were observed, which were not easy to determine, because the specimens were not very distinct.

Varieties of form.

It is not uncommon to meet with flattened quadrilateral prisms, the faces of which form angles of 101° 32′ and 78° 28′; angles that occur in several minerals, particularly in the calcareous spar. At the extremities of these prisms were faces in greater or less number, which we could not ascertain.

Not fissile.

As to its contexture, we could not find it split decisively in any direction.

Resembles the spinelle.

With respect to its crystallization it can be compared only with the spinelle, with which Mr. Haüy classes the ceylanite or pleonast. As analysis informs us too, that it resembles it in its constituent parts, we must consider them as similar.

The following is a comparative analysis of them.

Comparative analysis.

	Of the spinelle by Vauquelin.		Of the spinelle by Klaproth.	Of the ceylanite by Collet-Descotils.	Of the siderite by Trommsdorff.
Alumine ……	86·0	82·47	74·5	68	66
Magnesia ….	8·5	8·78	8.25	12	18
Silex ……..			15·5	2	10
Lime ……..			0·75		2
Oxide of iron ..			1·5	16	2·5
Oxide of chrome	5·25	6·18			

We find that alumine united with magnesia must be considered as the essential part of the mineral.

Its analysis.

As Mr. Bernhardi took upon himself to describe the characters of the lazulite, Mr. Trommsdorff attended more particularly to the analysis. He proceeded as follows.

Calcined.

A. A hundred grains of siderite strongly calcined in a covered crucible lost 5 grains of their weight. The fine blue colour had disappeared, and was changed to a yellowish white.

B. The

Treated with soda.

B. The calcined mineral was easily ground, and did not scratch the agate mortar. One hundred grains were urged to a red heat with 400 of caustic soda; and after a pasty fusion there remained a mass, which, diffused in water, afforded a turbid solution void of colour. This was supersaturated with muriatic acid; evaporated and redissolved in boiling water; when silex was precipitated from it, which weighed 10 grains after calcination.

Silex precipitated.

C. The boiling liquor was precipitated by carbonate of soda.

No glutine or yttria.

D. The precipitate, containing neither glucine nor yttria, was boiled in a lixivium of caustic soda, which effected a partial solution. The spongy, insoluble, brownish red residuum was set apart.

Lixivium saturated with muriatic acid, and alumine separated.

E. The soda lixivium (D) was supersaturated with muriatic acid, and the boiling liquor precipitated by carbonate of soda. The white precipitate, after sufficient elutriation, and being strongly calcined, left 66 grains of pure alumine.

Lime.

F. The reddish brown residuum (D) dissolved entirely in muriatic acid. The solution was concentrated, and the excess of muriatic acid saturated with ammonia. A little concentrated sulphuric acid was then poured in, which threw down a white precipitate. This was washed several times in cold water, and calcined, after which 6 grains of sulphate of lime, being equivalent to 2 grains of lime, remained.

Oxide of iron.

G. Into the liquor from which the lime had been precipitated prussiate of potash was poured, and the precipitate produced contained 2·5 grains of oxide of iron.

Magnesia.

H. The liquor decanted from the prussiate of potash was mixed with carbonate of soda, and kept some time boiling. A white substance fell down, which, after calcination, consisted of 18 grains of magnesia.

Its component parts.

One hundred grains of the calcined fossil therefore contained

Silex	10	(B)
Alumine	66	(E)
Magnesia	18	(H)
Lime	2	(F)
Oxide of Iron	2·5	(G.)
Loss	1·5	
	100	

The

The blue colour of the fossil appears to be owing to the degree of oxidation of the iron; and this is so much the more probable, as Mr. Ritter has announced the existence of a blue oxide of iron.

Blue colour owing to an oxide of iron.

It is true Mr. Guyton has discovered also a blue sulphuret of iron, to which he ascribes the colour of lapis lazuli: but in this case perhaps the sulphur may serve to produce this minimum of oxidation. Besides, direct experiments on the lazulite have convinced the author of this memoir, that it does not contain the least trace of sulphur or sulphuric acid.

Not a sulphuret as Guyton supposed.

VII.

On the Preparation of pure Barytes: by Mr. ROBIQUET*.

IN a note inserted in No. 183 of the Annales de Chimie, [see Journal No. 76, p. 66], on the decomposition of acetate of barytes by means of soda, Mr. d'Arcet points out as a more economical and certain process for procuring pure barytes, to decompose any barytic salt, particularly the muriate, by a caustic alkali. I conceive however, that the preference he gives to this process over that more generally employed, namely the decomposition of the nitrate by means of heat, is not well founded.

Mr. d'Arcet's process for obtaining pure barytes not preferable to the common.

If we consider the subject in an economical view, we find in both cases a soluble barytic salt is first to be formed: that in the first case we cannot employ liquors sufficiently concentrated, to prevent any barytes from remaining in a state of solution: that whatever precaution we take in preparing the caustic alkali by means of lime, a portion will always become carbonated, were it only during the processes of filtration; consequently there will be so much to be deducted from the quantity of barytes that might have been obtained: that besides, as the liquor must be shaken during the precipitation, a certain portion will then become carbonated: that a loss is occasioned by the washing likewise: and lastly, that

Comparison of them.

Losses in his way.

* Annales de Chimie, Vol. LXII, p. 61, April, 1807.

a great

a great deal more becomes carbonated by dissolving it afresh in boiling water. It is obvious, that all these deductions taken together will amount to a considerable sum, while in the decomposition of the nitrate we obtain the whole of the quantity it contained, which amounts to nearly half the weight of the dry salt; and that besides this process is neither difficult nor expensive, to those who know how to conduct it properly. The following are the precautions to be taken, to ensure its success.

None in the other.]

Process for decomposing the nitrate.

Let a covered crucible be nearly two thirds filled with dry and powdered nitrate of barytes, and placed in a common furnace, heated moderately so as to cause the salt to dissolve in its own water of crystallization. Increase the fire gradually, and with caution, on account of the considerable tumefaction that takes place toward the end. When the mass, which ought then to be of a cherry red, no longer emits any bubbles, cover the crucible with charcoal to the depth of an inch or two; fit on the furnace its dome, furnished with a plate iron chimney; let it heat thus for a quarter of an hour; and afterward withdraw the crucible from the fire, break it, and put the barytes into a close vessel as quickly as possible.

7lbs. produced 3lbs 6oz. of pure barytes.

In this way I lately treated seven pounds of nitrate, which I divided into three common crucibles, and placed in the same furnace. The charcoal expended cost about 30s. [1s. 3d.]; the decomposition was completely effected in two hours; and I obtained 3lbs. 6oz. of perfectly pure barytes. But it is to be observed, that, if the barytes be kept too long in the fire after the nitrate is decomposed, it will become considerably carbonated: and if the quantity be at all too great, it is impossible, whatever heat we afterward employ, to deprive it completely of carbonic acid. This is the whole of the difficulty, which is completely removed, by acting as I have directed.

Necessary caution.

Advantages of this mode.

Thus I conceive it is in reality more economical, to extract the barytes from the nitrate by the help of fire, than to follow the process proposed by Mr. d'Arcet: for even supposing the barytes to be equal in quantity by both processes, which I have shown cannot be the case, the price of the potash I must have employed would have nearly doubled the

the expense. And as to the purity of the product, since the washing must be performed very sparingly, I do not see, that the process of Mr. d'Arcet deserves the preference in this respect: for it is probable, that the barytes thus obtained will retain a little of the salt of the mother water; and on the contrary, that obtained from the nitrate is extremely pure, at least if the precaution be taken, before it is decomposed, to calcine it slightly, and redissolve it, in order to separate a portion of iron proceeding from the sulphate employed.

VIII.

Remark on the spontaneous Decomposition of the hidroguretted Sulphuret of Barytes: by Messrs. ROBIQUET *and* CHEVREUL *.

IN the course of last month, Mr. Robiquet, in order to separate some crystals, that had formed in a phial half filled with hidroguretted sulphuret of barytes, turned it upside down, without uncorking it. Some days after, the weather having grown colder, the liquor afforded some tolerably large crystals, which were of a very different figure from those, that remained at the bottom of the phial. We have examined these two substances together, and the following are the results of our observations.

Two sorts of crystals formed spontaneously in hidroguretted sulphuret of barytes.

1. The first crystals were elongated prisms. On the application of sulphuric acid they gave out sulphurous acid gas, and at the same time let fall sulphur mixed with sulphate of barytes. Hence there could be no doubt, that they were sulphuretted sulphite of barytes.

1st, sulphuretted sulphite of barytes.

2. The mother water, in which the second crystals had formed, was colourless and very limpid. It retained neither sulphur nor sulphurous acid; had all the characters of a simple solution of barytes in water; and the crystals comported themselves as the crystals of that earth. They dissolved in weak muriatic acid without effervescence, and in

2d, pure barytes, in barytes water.

* Annales de Chimie, vol. LXII, p. 180, May, 1807.

water

water without leaving any residuum. The latter solution yielded a precipitate both with sulphuric and with carbonic acid.

Occasioned by the oxigen in the phial.

From these observations it was easy to explain the separation of the hidroguretted sulphuret of barytes into pure barytes and sulphuretted sulphite. The oxigen contained in the phial, being absorbed by the sulphuret, formed water and sulphurous acid: but the quantity of oxigen being insufficient, to convert all the sulphuret into sulphite, the consequence was, that the portion of sulphite which was formed sulphuretted itself at the expense of the undecomposed sulphuret, and left its base free. The sulphuretted sulphite, being less soluble than the barytes, of course crystallized first.

Hidroguretted sulphurets, absorbing oxigen gas, always form sulphites.

Sulphite of barytes takes sulphur from barytes.

Hence we conclude, that the absorption of oxigen gas by hidroguretted sulphurets never produces immediately a sulphate, but a sulphite, notwithstanding the great affinity of the base for sulphuric acid; as Mr. Berthollet has explained in his Memoir on sulphuretted hidrogen: and that the affinity of sulphite of barytes for sulphur is greater, than that of barytes for the same substance.

IX.

Remark on a Property of Camphorated Water: by C. A. CADET, *Apothecary in ordinary to his Majesty**.

Carbonic acid said to promote the solution of camphor in water.

A Surgeon at Madrid announced three years ago, that carbonic acid promoted the solution of camphor in water, and that this water had very decided medicinal properties in disorders of the bladder. Leaving to the physician to determine the value of the medicine, I have attempted merely to confirm the chemical fact.

Water alone dissolved $\frac{1}{560}$, aerated water only $\frac{1}{1034}$.

For this purpose I made a solution of camphor in distilled water, and another in water saturated with carbonic acid after Mr. Paul's method, in order to compare the quantities of camphor dissolved. I weighed the camphor before and

* Annales de Chimie, vol. LXII, p. 132, May, 1807.

after

after solution, and I found, that the distilled water had taken up sixteen grains per quart, and the carbonic acid only fifteen. As I had been obliged to filter the liquors and dry the filters, I imagined, that the undissolved camphor must have lost some of its weight by evaporation, and that the balance did not give me the precise quantity absorbed by the water. Accordingly I sought for a reagent, that should acquaint me with the presence of camphor in water.

Perhaps an error from evaporation in drying.

Potash I found would precipitate camphorated water, while neither soda nor ammonia rendered it at all turbid. But the potash must be pure and caustic. If it contain carbonic acid it no longer precipitates the camphor: and if, after it has been precipitated, the vessel be left exposed to the air, the liquid recovers its transparency by absorbing carbonic acid.

Pure potash precipitates camphor from water, but no other alkali does.

Here then we have a new method of distinguishing potash from soda. Camphorated water is in this respect a more certain test than the nitromuriate of platina, and more easily procured. The metallic salt however is more commodious, as it precipitates the carbonate of potash.

This a new test to distinguish potash from soda.

When employing caustic potash as a test of camphorated water impregnated with carbonic acid, I obtained no precipitate but by adding a great excess of alkali; and this precipitate did not appear to me more considerable, than that obtained in distilled water. I think therefore, that carbonic acid does not in any sensible degree promote the solution of camphor in water: and it follows at least from these experiments, that water does not impregnate itself with the aroma of the camphor solely, as some chemists have believed, but that it dissolves a sufficient proportion of this concrete volatile oil for the purposes of which it is employed.

Pure potash in excess precipitates it if carbonic acid be present.

If the camphor be reduced to a state of extreme division by trituration with a few drops of alcohol, the water will take up more than sixteen grains per quart: some chemists have dissolved as far as thirty grains.

If the camphor be divided by alcohol 1 qt. will take up 30 grains.

X.

X.

Report on a Memoir of Mr. Destouches, *Apothecary at Paris; by Messrs.* Vauquelin *and* Boullay. *Read at the Parisian Society of Pharmacy, Feb.* 16, 1807 *.

THE paper, on which these gentlemen were appointed to make a report, was entitled, a Memoir on the Tartrite of Lime contained in the Tartarous Acidule.

In preparing tartarised natron very little tartrite of lime found.

Preparing Rochelle salt in quantity, Mr. Destouches was desirous of collecting the tartrite of lime, that separates from cream of tartar at the moment of its saturation, in order to turn it to account: but he was very much surprised not to obtain more than two pounds of precipitate at farthest from about three hundred of cream of tartar, that he had used, instead of ten times that quantity, which he had reason to expect from the observations of Mr. Vauquelin.

Repeated with the same effect.

The same process repeated afforded Mr. Destouches but a very slight precipitate, which, confirming the former, induced him to make the following experiments.

Exp. 1. About 10 oz. of cream of tartar gave 1 dr. of tartrite of lime.

1st. To a boiling solution of eight ounces of crystallized carbonate of soda he added cream of tartar to the point of saturation, without any precipitate being produced: but after the solution had stood twenty-four hours, a number of silky crystals were deposited, which when separated weighed five drachms. These crystals, being mixed with an excess of acidulous tartrite of potash, were reduced to one drachm by washing with boiling water.

Exp. 2. Apparently but 18 grs.

2d. A fresh experiment, made with the cream of tartar employed in the operations in the large way, afforded but two drachms of precipitate, which were reduced to eighteen grains by washing with boiling water.

Tartarised natron promotes the solution of tartrite of lime by boiling, but it falls down on standing.

Surprised by these results, Mr. Destouches conceived, that the tartrite of lime might be dissolved by the Rochelle salt, which prevented it from separating readily. In consequence he boiled a pound of Rochelle salt and two ounces of tartrite of lime in two quarts of water, when three drachms

* Annales de Chimie, vol. LXII, p. 33. April, 1807.

of

of the calcareous salt were dissolved; but, after standing two da s, the whole was deposited in a needly form, so as not to show an atom of lime on the addition of oxalate of ammonia.

Whence could arise this difference in the quantity of tartrite of lime in different parcels of cream of tartar, which, according to Mr. Destouches, was $\frac{1}{26}$ in the first experiment, and $\frac{1}{100}$ in the second?

Supertartrite of potash takes up $\frac{1}{16}$ of tartrite of lime by boiling them together.

To account for this fact, and ascertain whether, if the acidulous tartrite of potash contained little or no tartrite of lime, it might acquire some in the process of purification, the author boiled two ounces of tartrite of lime and eight of cream of tartar in eight quarts of water. In this process the latter retained $\frac{1}{16}$ of its weight of the former.

Mr. Destouches farther satisfied himself of the proportion in which the tartrite of lime unites with boiling water.

General conclusions.

Lastly, he concluded from his experiments:

The calcareous salt variable.

1st, That the quantity of tartrite of lime in cream of tartar is liable to vary from the smallest quantity to seven per cent.

Soluble in 600 parts of boiling water.

2dly, That tartrite of lime is soluble in six hundred parts of boiling water; and that it is susceptible of a regular crystallization by being dissolved in a soluble tartrite.

In making tartarised natron the solution should be suffered to cool.

3dly, That, in making Rochelle salt, the solution should be suffered to cool, in order to deprive it of tartrite of lime.

Carbonate of soda a test of lime in cream of tartar.

4thly, That the carbonate of soda affords the most simple means of analysing cream of tartar with respect to tartrite of lime*.

Experiments by Vauquelin and Boullay.

Experiments and reflections by the commissioners.

Six parcels of cream of tartar left different proportions of tartrite of lime,

1. Six parcels of cream of tartar of the shops, bought at different places, were numbered. A hundred drachms of each, saturated hot with a solution of pure carbonate of soda, exhibited towards the end of the saturation a greater or less quantity of precipitate, which separated spontaneously, but only toward the end of the combination. The solutions, filtered separately, as soon as they were cooled, left on the filter a substance, part of which was crystalline, part pulverulent, in the following proportions.

Cream

* See page 32.

thirty-five hundredth parts of the whole. Consequently the specimens of cream of tartar, which were the object of our inquiries, contained the following quantities of lime;

Proportions of lime in the cream of tartar:

	grs.		grs.		grs.
No. 1 —	17·82,	No. 3 —	17·28,	No. 5 —	20·52,
2 —	21·68,	4 —	21·06,	6 —	17·28:

and therefore of tartrite of lime;

and hence of tartrite.

	grs.		grs.		grs.
No. 1 —	50·91,	No. 3 —	49·37,	No. 5 —	58·63,
2 —	61·94,	4 —	60·17,	6 —	49·37.

General inferences.

From all these facts we conclude:

Proportion of tartrite of lime from ·05 to ·07.

1st. That it is true, that the quantity of tartrite of lime varies in different parcels of cream of tartar to be met with in the shops: but that this variation does not exceed from five to seven per cent, at least in those we had an opportunity of examining.

Exists in the crude tartar.

2dly, That it is more natural to look for the source of this earthy salt in the crude tartar, which contains it ready formed, than to suppose it produced in the process of purifying it.

Carbonate of soda not a good test.

3dly, That the carbonate of soda does not appear by any means calculated for the analysis of cream of tartar with respect to tartrite of lime.

Tartarised natron retains a portion.

4thly, That in fact Rochelle salt promotes the solution of this calcareous salt with the assistance of heat; and it has the farther inconvenience of retaining a certain quantity a long time in solution.

Should be freed from it by cold solution.

5thly, That the Rochelle salt of the shops always contains more or less of this earthy salt, and that it ought to be redissolved in cold water, to obtain it perfectly pure.

6th, That the mode of analysis we employed appeared to us very proper, to make known precisely how much tartrite of lime is contained in the acidulous tartrite of potash.

XI.

XI.

Mineralogical Description and chemical Analysis of a Stone, called Pyrophysalite: by Messrs. HISINGER *and* BERZELIUS*.

THE colour of this stone is white, or sometimes of a greenish white, and occasionally small superficial blue spots of fluate of lime may be observed on it. Colour.

It is found in masses, forming oblong nodules, most commonly of no determinate figure, but sometimes approaching an irregular rhomboid. Hence no exact measure of its angles can be taken: though apparently its lateral angles are about 118° and 62° reciprocally. Form.

Its fracture is unequal, foliated and very shining in one direction only, which seems to be that formed by the inclination of 90° or 100° to the axis of the rhomboid. It may be cleft, though less decidedly, in two other directions nearly parallel to the sides of the rhomboid. If broken transversely, it has little or no lustre. The fragments are of an indeterminate form, angular, with sharp edges, on which it is a little translucid. They strike fire with steel, and are hard enough to scratch glass easily, but are scratched by quartz. It is difficult to reduce to powder. Specific gravity 3·451. Fracture. Hardness and gravity.

The powder of the purest fragments, projected into a hot spoon, emit a greenish phosphoric light, that is but of short duration. Phosphorescent by heat.

Before the plowpipe without any addition it is nearly infusible: but if the heat be urged to a high degree, it renders it white, opake, and its surface is surrounded by small bubbles, which issue from it hastily, and burst if the temperature be kept up. This is a very decided characteristic appearance, from which the substance has received its name. Before the blowpipe nearly infusible, but at a high heat emits bubbles.

With borax it fuses easily into a colourless transparent glass. Fuses with borax.

* Annales de Chimie, vol LXVIII, p. 113, May, 1806.

Attacked by soda.

Soda attacks it with a little effervescence, and produces a porous mass.

Where found.

This stone was found by Mr. Gahn, at Finbo, near Fahlun, about three quarters of a league west of the town, on the road to Sundborn. The nodules are imbedded in a granite composed of white quartz, feldtspar, and silvery mica, the laminæ of which are rhomboidal and in hexagonal prisms. The nodules are separated from the rock by thin scales of mica, covered by a talcous substance of a greenish yellow colour.

Its difference from feldtspar.

It differs from feldtspar, to which it appears to have most resemblance, in having but one determinate direction in which it can be split, while feldtspar has two. The specific gravity of feldtspar too is but 2·704, and besides it is much less difficult to fuse.

Analysis.

The following analysis was undertaken conjointly with Mr. Berzelius.

Powdered.

Two hundred grains of pyrophysalite, reduced to fine powder in a mortar, acquired an increase of weight of four grains.

Heated alone.

a. These 204 grains, having been kept at a red heat in the fire for three hours, lost 1·5 grains.

Treated with carbonate of potash, and muriatic acid.

b. On adding 600 grains of carbonate of potash, and exposing the mixture to a red heat for three hours in a platina crucible, a colourless mass was obtained, perfectly soluble in muriatic acid. This solution being evaporated to dryness, and diffused in water with a very little muriatic acid, the silex was obtained, which, after having been washed and heated red hot for half an hour, weighed 66·25 grains.

Silex.

Precipitated by carbonate of potash.

c. The solution in water was precipitated by carbonate of potash, which was added in excess, taking care to keep the liquor boiling during the process. The precipitate obtained was dissolved in caustic potash, except a small portion of a yellowish powder.

Neither glucine, zircon, nor yttria.

d. To the liquor precipitated by carbonate of potash muriatic acid was added in excess, and caustic ammonia, without the liquor undergoing any change: a proof, that it contained neither glucine, zircon, nor yttria.

Muriate of ammonia added to

e. To the solution in caustic potash muriate of ammonia was added, and it was boiled till the ammonia was expelled in

in vapour. The alumine obtained by this process was carefully washed, and heated red hot. In this last operation, when the incandescence was carried to a high degree, the mass emitted fuming vapours; an unexpected phenomenon, that did not take place at a less elevated temperature. As we conceived these vapours to be muriate of ammonia, part of which might have remained in the mass, it was heated red hot in the fire full two hours longer. After this the alumine weighed 107·5 grains. In another experiment, when alumine had been exposed to a lower degree of heat, and for a quarter of an hour only, 116 grains were obtained, which were reduced to 107·5 by longer calcination. In these operations an aluminous salt was found to attach itself to the edges of the lid that covered the crucible, but the smallness of its quantity did not allow us to examine its nature. Another time, instead of exposing the alumine to heat, we dissolved it in sulphuric acid, and added a little potash; when the result was a crystallization of sulphate of alumine, which continued to the last drop. The sulphuric acid, in dissolving the alumine, left a residuum of 2 grains of silex.

the alkaline solution & boiled,

Alumine.

An aluminous salt.

Silex.

f. The yellow powder, which was not attacked by the caustic potash (*c*), was dissolved in nitromuriatic acid; being evaporated to dryness, and redissolved in water, a grain and half of silex were separated from it. By adding to the liquor succinate of ammonia, a precipitate of oxide of iron, weighing 1·75 grain, was obtained: and on adding caustic ammonia 1 grain of alumine was precipitated. The remaining liquor being boiled with carbonate of potash, some carbonate of lime was separated, which, after being heated red hot in the fire, weighed 1·75 grain. This portion of lime dissolved in weak sulphuric acid without effervescence forming with it sulphate of lime.

Yellow residuum.

Silex.

Oxide of iron.

Alumine.

Lime.

Thus, if we subtract the 4 grains of silex gained from the mortar in reducing the stone to powder, we find the proportions given by 100 parts of it to be

Component parts.

Alumine	53·25
Silex	32·88
Lime	0·88
Oxide of iron	0·88
	87·89
Loss by calcination	0·75
Loss in the analysis	11·36
	100

Loss appeared not to proceed from an alkali.

The last mentioned loss, which we experienced in several trials, led us to suspect the presence of an alkali. In consequence we heated the stone with nitrate of barytes, dissolved in sulphuric acid the mass obtained by this operation, and poured ammonia into the solution. The saline liquor being evaporated, and the salt heated red hot in a platina crucible, we imagined in what remained we discovered traces of a salt with an alkaline base, mixed with sulphate of lime, but the quantity of which was too small to ascertain its weight. It is even probable, that this salt may have been produced by the reagents. Thus it remained for us to examine, whether this stone did not contain an acid; as the fluoric for instance.

Examined for an acid.

In order to determine this, we saturated with muriatic acid the liquor that remained after the precipitation of the earthy substances in the preceding experiments, and then added muriate of lime. No precipitate however was obtained. We then determined to boil for an hour a portion of the stone, previously reduced to powder, in sulphuric acid. Employing a glass retort in this operation, we placed a vessel filled with lime-water, to receive the gasses, that should pass over during the solution. None however came over, except what was contained in the vessels, and the lime-water underwent no alteration. We saw however that the upper part of the retort and part of the receiver had been corroded by fluoric acid. This acid therefore actually exists in the stone, though perhaps in small quantity, or strongly united with its base. Mr. J. G. Gahn observed a more considerable extrication of it, by treating with sulphuric acid the powder of this stone previously fused with an alkali. In our

It contains the fluoric.

our experiment with nitrate of barytes, this change could scarcely be perceived. Hence we have still a suspicion, that the fluoric acid, which adheres strongly to alumine, may have carried off a portion of this earth with it at a high temperature, as was observed by Mr. Klaproth in his experiments on the topaz. In our experiments therefore there may have been a loss of both fluoric acid and alumine at the same time.

Perhaps carried off some alumine with it.

Finally we conceive the presence of fluoric acid will explain that striking emanation of bubbles, which is exhibited by this stone when exposed to the flame of the blowpipe: it appears, that part of the acid united to its earthy base produces a very fusible substance, while another is extricated in the forms of vapour. This supposition is strengthened by the observation of Mr. Gahn, that the topaz, particularly that of Brasil, when exposed to a very violent heat, emits bubbles similar to those produced on the pyrophysalite. As the topaz contains alumine and silex, with a portion of fluoric acid, we conceive it ought to be placed in a mineralogical view between the topaz and the pycnite, which, according to Mr. Bucholz, contains 0·17 of fluoric acid*.

This accounts for the bubbles.

Topaz emits the same.

Its place among minerals.

XII.

On some Chemical Agencies of Electricity, by HUMPHREY DAVY, *Esq. F. R. S. M. R. I. A.*

Concluded from Vol. XVIII, p. 339.

V. *On the Passage of Acids, Alkalis, and other Substances through various attracting chemical Menstrua, by Means of Electricity.*

AS acid and alkaline substances, during the time of their electrical transfer, passed through water containing vegetable colours

Passage of various substances through at-

* According to Vauquelin the pycnite, schorlite of Klaproth and others, schorliform beryl of some, contains but 0·06 of fluoric acid, 0·60 alumine, 0·30 silex, 0·02 lime, and 0·01 water. Haüy thinks, that the pyrophysalite should be considered as a variety of the topaz. Ed.

tracting chemical mixtures by means of electricity.

colours without affecting them, or apparently combining with them, it immediately became an object of inquiry, whether they would not likewise pass through chemical menstrua, having stronger attractions for them; and it seemed reasonable to suppose, that the same power, which destroyed elective affinity in the vicinity of the metallic points, would likewise destroy it, or suspend its operation, throughout the whole of the circuit.

An arrangement was made, of the same vessels and apparatus employed in the experiment on the solution of muriate of soda and sulphate of silver, vol. XVIII, p. 338. Solution of sulphate of potash was placed in contact with the negatively electrified point, pure water was placed in contact with the positively electrified point, and a weak solution of ammonia was made the middle link of the conducting chain; so that no sulphuric acid could pass to the positive point in the distilled water, without passing through the solution of ammonia.

The power of 150 was used: in less than five minutes it was found, by means of litmus paper, that acid was collecting round the positive point; in half an hour, the result was sufficiently distinct for accurate examination.

The water was sour to the taste, and precipitated solution of nitrate of barytes.

Similar experiments were made with solution of lime, and weak solutions of potash and soda, and the results were analogous. With strong solutions of potash and soda a much longer time was required for the exhibition of the acid; but even with the most saturated alkaline lixivium, it always appeared in a certain period.

Muriatic acid, from muriate of soda, and nitric acid from nitrate of potash, were transmitted through concentrated alkaline menstrua, under similar circumstances.

When distilled water was placed in the negative part of the circuit, and a solution of sulphuric, muriatic, or nitric acid, in the middle, and any neutral salt with a base of lime, soda, potash, ammonia, or magnesia, in the positive part, the alkaline matter was transmitted through the acid matter to the negative surface, with similar circumstances to those occurring during the passage of the acid through the alkaline menstrua;

strua; and the less concentrated the solution, the greater seemed to be the facility of transmission.

Passage of various substances through attracting chemical mixtures by means of electricity.

I tried in this way muriate of lime with sulphuric acid, nitrate of potash with muriatic acid, sulphate of soda with muriatic acid, and muriate of magnesia with sulphuric acid; I employed the power of 150; and in less than 48 hours, I gained in all these cases decided results; and magnesia came over like the rest.

Strontites and barytes passed, like the other alkaline substances, readily through muriatic and nitric acids; and, *vice versâ*, these acids passed with facility through aqueous solutions of barytes and strontites; but in experiments in which it was attempted to pass sulphuric acid through the *same menstrua*, or to pass barytes or strontites through this acid, the results were very different.

When solution of sulphate of potash was in the negative part of the circuit, distilled water in the positive part, and saturated solution of barytes in the middle, no sensible quantity of sulphuric acid existed in the distilled water after 30 hours, the power of 150 being used; after four days, sulphuric acid appeared, but the quantity was extremely minute; much sulphate of barytes had formed in the intermediate vessel; the solution of barytes was so weak as barely to tinge litmus; and a thick film of carbonate of barytes had formed on the surface of the fluid. With solution of strontites the result was very analogous, but the sulphuric acid was sensible in three days.

When solution of muriate of barytes was made positive by the power of 150, concentrated sulphuric acid intermediate, and distilled water negative: no barytes appeared in the distilled water, when the experiment had been carried on for four days; but much oximuriatic acid had formed in the positive vessel, and much sulphate of barytes had been deposited in the sulphuric acid.

Such of the metallic oxides as were made subjects of experiment passed through acid solutions from the positive to the negative side, but the effect was much longer in taking place than in the instances of the transition of alkaline matter. When solution of green sulphate of iron was made positive,

solution

Passage of various substances through attracting chemical mixtures by means of electricity.

solution of muriatic acid intermediate, and water negative, in the usual arrangement, green oxide of iron began to appear in about ten hours upon the negative connecting amianthus, and in three days a considerable portion had been deposited in the tube. Analogous results were obtained with sulphate of copper, nitrate of lead, and nitromuriate of tin.

I made several experiments on the transition of alkaline and acid matter through different neutrosaline solutions, and the results were such as might well have been anticipated.

When solution of muriate of barytes was negative, solution of sulphate of potash intermediate, and pure water positive, the power being from 150, sulphuric acid appeared in about five minutes in the distilled water; and in two hours the muriatic acid was likewise very evident. When solution of sulphate of potash was positive, solution of muriate of barytes intermediate, and distilled water negative, the barytes appeared in the water in a few minutes; the potash from the more remote part of the chain was nearly an hour in accumulating, so as to be sensible.

When the solution of muriate of barytes was positive, the solution of sulphate of potash intermediate, and distilled water negative, the potash soon appeared in the distilled water; a copious precipitation of sulphate of barytes formed in the middle vessel; but after ten hours no barytes had passed into the water.

When solution of sulphate of silver was interposed between solution of muriate of barytes on the negative side, and pure water on the positive side, sulphuric acid alone passed into the distilled water; and there was a copious precipitation in the solution of sulphate of silver. This process was carried on for ten hours.

I tried several of these experiments of transition upon vegetable and animal substances with perfect success.

The saline matter exposed in contact with the metal, and that existing in the vegetable or animal substances, both underwent decomposition and transfer; and the time of the appearance of the different products at the extremities of the circuit was governed by the degree of their vicinity.

Thus, when a fresh leaf stalk of the polyanthus, about 2 inches

Passage of various substances through attracting chemical mixtures by means of electricity.

inches long, was made to connect a positively electrified tube containing solution of nitrate of strontites, and a negatively electrified tube containing pure water; the water soon became green, and gave indications of alkaline properties, and free nitric acid was rapidly separated in the positive tube. After ten minutes, the alkaline matter was examined; it consisted of potash and lime, and as yet no strontites had been carried into it: for the precipitate it gave with sulphuric acid readily dissolved in muriatic acid. In half an hour strontites, however, appeared; and in four hours it formed a very abundant ingredient of the solution.

A piece of muscular flesh of beef, of about 3 inches in length and half an inch in thickness, was treated in the same way as the medium of communication between muriate of barytes and distilled water. The first products were soda, ammonia, and lime; and after an hour and a quarter, the barytes was very evident. There was much free oximuriatic acid in the positively electrified tube, but no particle of muriatic acid had passed into the negative tube, either from the muriatic solution or from the muscular fibre.

VI. *Some general Observations on these Phænomena, and on the Mode of Decomposition and Transition.*

General observations on the preceding phenomena.

It will be a general expression of the facts that have been detailed, relating to the changes and transitions by electricity, in common philosophical language, to say, that hidrogen, the alkaline substances, the metals, and certain metallic oxides, are attracted by negatively electrified metallic surfaces, and repelled by positively electrified metallic surfaces; and contrariwise, that oxigen and acid substances are attracted by positively electrified metallic surfaces, and repelled by negatively electrified metallic surfaces; and these attractive and repulsive forces are sufficiently energetic, to destroy or suspend the usual operation of elective affinity.

It is very natural to suppose, that the repellent and attractive energies are communicated from one *particle to another particle* of the same kind, so as to establish a conducting chain in the fluid; and that the locomotion takes place in consequence;

General observations on the preceding phenomena.

consequence; and that this is really the case seems to be shown by many facts. Thus, in all the instances in which I examined alkaline solutions through which acids had been transmitted, I always found acid in them whenever any acid matter remained at the original source. In time, by the attractive power of the positive surface, the decomposition and transfer undoubtedly become complete; but this does not affect the conclusion.

In the cases of the separation of the constituents of water, and of solutions of neutral salts forming the whole of the chain, there may possibly be a succession of decompositions and recompositions throughout the fluid. And this idea is strengthened by the experiments on the attempt to pass barytes through sulphuric acid, and muriatic acid through solution of sulphate of silver, in which, as insoluble compounds are formed and carried out of the sphere of the electrical action, the power of transfer is destroyed. A similar conclusion might likewise be drawn from many other instances. Magnesia and the metallic oxides, as I have already mentioned, will pass along moist amianthus from the positive to the negative surface; but if the vessel of pure water be interposed, they do not reach the negative vessel, but sink to the bottom. These experiments I have very often made, and the results are perfectly conclusive; and in the case, page 39, in which sulphuric acid seemed to pass in small quantities through very weak solutions of strontites and barytes, I have no doubt but that it was carried through by means of a thin stratum of pure water, where the solution had been decomposed at the surface by carbonic acid; for in an experiment similar to these in which the film of carbonate of barytes was often removed and the fluid agitated, no particle of sulphuric acid appeared in the positive part of the chain.

It is easy to explain, from the general phænomena of decomposition and transfer, the mode in which *oxigen* and *hidrogen* are separately evolved from water. The oxigen of a portion of water is attracted by the positive surface, at the same time that the other constituent part, the hidrogen, is repelled by it; and the opposite process takes place at the negative surface; and in the middle or neutral point of the circuit,

circuit, whether there be a series of decompositions and recompositions, or whether the particles from the extreme points only are active, there must be a new combination of the repelled matter: and the case is analogous to that of two portions of muriate of soda separated by distilled water; muriatic acid is repelled from the negative side, and soda from the positive side, and muriate of soda is composed in the middle vessel.

General observations on the preceding phenomena.

These facts seem fully to invalidate the conjectures of M. Ritter, and some other philosophers, with regard to the elementary nature of water, and perfectly to confirm the great discovery of Mr. Cavendish.

M. Ritter conceived, that he had procured oxigen from water without hidrogen, by making sulphuric acid the medium of communication at the negative surface; but in this case, sulphur is deposited, and the oxigen from the acid, and the hidrogen from the water, are respectively repelled; and a new combination produced.

I have attempted some of the experiments of decomposition and transfer, by means of common electricity, making use of a powerful electrical machine of Mr. Nairne's construction, belonging to the Royal Institution, of which the cylinder is 15 inches in diameter, and 2 feet long.

With the same aparatus as that employed for decompositions by the Voltaic battery, no perceptible effect was produced by passing a strong current of electricity silently for four hours through solution of sulphate of potash.

But by employing fine platina points of $\frac{1}{70}$ of an inch in diameter, cemented in glass tubes in the manner contrived by Dr. Wollaston*, and bringing them near each other, in vessels containing from 3 to 4 grains of the solution, and connected by moist asbestus, potash appeared in less than two hours round the negatively electrified point, and sulphuric acid round the positive point.

In a similar experiment sulphuric acid was transferred through moist asbestus into water; so that there can be no

* Phil. Trans. Vol. XCI, page 427.

doubt

doubt, that the principle of action is the same in common and the Voltaic electricity*.

VII. *On the general Principles of the chemical Changes produced by Electricity.*

General principles of the chemical changes produced by electricity.

The experiments of Mr. Bennet had shown, that many bodies brought into contact and afterwards separated, exhibited *opposite* states of electricity; but it is to the investigations of Volta that a clear developement of the fact is owing; he has distinctly shown it in the case of copper and zinc, and other metallic combinations; and has supposed that it also takes place with regard to metals and fluids.

In a series of experiments made in 1801†, on the construction of electrical combinations by means of alternations of single metallic plates, and different strata of fluids, I observed, that, when acid and alkaline solutions were employed as elements of these instruments, the alkaline solutions always received the electricity from the metal, and the acid always transmitted it to the metal; thus, in an arrangement of which the elements were tin, water, and solution of potash, the circulation of the electricity was from the water to the

* This had been shown, with regard to the decomposition of water, by Dr. Wollaston's important researches.—By carefully avoiding sparks, I have been able to obtain the two constituents in a separate state. In an experiment in which a fine platina point cemented in glass, and connected by a single wire with the positive conductor of this machine, was plunged in distilled water in an insulated state, and the electricity dissipated into the atmosphere by means of moistened filaments of cotton, oxigen gas, mixed with a little nitrogen gas, was produced; and when the same apparatus was applied to the negative conductor hidrogen gas was evolved, and a minute portion of oxigen and nitrogen gasses; but neither of the foreign products, the nitrogen gas in the one case and the nitrogen and oxigen gasses in the other, formed as mush as $\frac{1}{10}$ part of the volume of the gasses; and there is every reason to suppose, that they were derived from the extrication of common air, which had been dissolved in the water. This result, which, when I first obtained it in 1803, appeared very obscure, is now easily explained; the alternate products must have been evolved at the points of the dissipation of the electricity.

† See Phil. Trans. Vol. XCI, page 397.

tin,

tin, and from the tin to the solution of potash; but in an arrangement composed of weak nitric acid, water, and tin; the order was from the acid to the tin, and from the tin to the water.

General principles of the chemical changes produced by electricity.

These principles seem to bear an immediate relation to the general phænomena of decomposition and transference, which have been the subject of the preceding details.

In the simplest case of electrical action, the alkali which receives electricity from the metal would necessarily, on being separated from it, appear positive; whilst the acid under similar circumstances would be negative; and these bodies having respectively, with regard to the metals, that which may be called a positive and a negative electrical energy, in their repellent and attractive functions seem to be governed by laws the same as the common laws of electrical attraction and repulsion. The body possessing the positive energy being repelled by positively electrified surfaces, and attracted by negatively electrical surfaces; and the body possessing the negative energy following the contrary order.

I have made a number of experiments with the view of elucidating this idea, and of extending its application; and in all cases they have tended to confirm the analogy in a remarkable manner.

Well burned charcoal, water, and nitric acid; the same substance, water, and solution of soda; made respectively elements of different electrical combinations, became distinctly active when 20 alternations were put together: the positive energy being exhibited on the side of the alkali, and the negative on that of the acid. Arrangements of plates of zinc, pieces of moistened pasteboard, and moistened quicklime, to the number of 40 series, likewise formed a weak electrical pile, the effect of the lime being similar to that of an alkali, but the power was soon lost.

I endeavoured, by means of very delicate instruments, to ascertain the electrical states of single insulated acid and alkaline solutions, after their contact with metals; and for this purpose I employed at different times the condensing electrometer of Mr. Cuthbertson's construction, Mr. Cavallo's multiplier, and a very sensible electrical balance, on the principle of tortion, adopted by M. Coulomb; but the effects

General principles of the chemical changes produced by electricity.

effects were unsatisfactory, the circumstances of evaporation, and of chemical action, and the adherence of the solutions to the surfaces of the metals employed, in most cases, prevented any distinct result, or rendered the source of the electricity doubtful. I shall not enter into any details of these processes, or attempt to draw conclusions from capricious and uncertain appearances, which, as we shall immediately see, may be fully deduced from clear and distinct ones.

The alkaline and acid substances capable of existing in the dry and solid form, give by contact with the metals exceedingly sensible electricities, which require for their exhibition the gold leaf electrometer only with the small condensing plate.

When oxalic, succinic, benzoic, or boracic acid, perfectly dry, either in powder or crystals, was touched upon an extended surface with a plate of copper insulated by a glass handle, the copper was found positive, the acid negative. In favourable weather, and when the electrometer was in perfect condition, one contact of the metal was sufficient to produce a sensible charge; but seldom more than five or six were required. Other metals, zinc and tin for instance, were tried with the same effect. And the metal received the positive charge, apparently to the same extent, whether the acid was insulated upon glass, or connected with the ground.

The solid acid of phosphorus, which had been strongly ignited, and most carefully excluded from the contact of air, rendered the insulated plate of zinc positive by four contacts; but after exposure to the atmosphere for a few minutes it wholly lost this power.

When metallic plates were made to touch dry lime, strontites, or magnesia, the metal became negative; the effect was exceedingly distinct, a single contact upon a large surface being sufficient to communicate a considerable charge. For these experiments the earths were carefully prepared; they were in powder, and had been kept for several days in glass bottles before they were used: it is essential to the success of the process that they be of the temperature of the atmosphere. In some experiments which I made upon them when cooling, after having been ignited; they appeared strongly electrical,

electrical, and rendered the conductors brought in contact with them positive.

General principles of the chemical changes produced by electricity.

I made several experiments in a similar manner on the effects of the contact of potash and soda with the metals. Potash in no instance afforded a satisfactory result; its powerful attraction for water presents an obstacle probably unsurmountable to the success of any trials made in the free atmosphere. Soda, in the only case in which electricity was exhibited, affected the metal in the same way as lime, strontites, and magnesia. Upon this occasion the soda had been prepared with great care, exposed in a platina crucible for nearly an hour in a red heat, and suffered to cool in the crucible inverted over mercury: when cool it was immediately removed, and the contact made with a plate of zinc: the experiment was performed in the open air; the weather was peculiarly dry, the thermometer stood at 28° Fahrenheit, and the barometer at 30·2 inches: six contacts gave a charge to the condensing electrometer in the first trial; in the second ten were required to produce a similar effect; and after this, though two minutes only had elapsed, no further result could be obtained.

In the decomposition of sulphuric acid by Voltaic electricity the sulphur separates on the negative side. The experiments of various electricians prove, that, by the friction of sulphur and metals, the sulphur becomes positive and the metals negative; the same thing I find happens from the contact of an unexcited cake of sulphur and insulated metallic plates. Mr. Wilke has stated an exception to lead, as rendering sulphur negative by its friction. The results that I have obtained with lead, in trials very carefully made, are the same as those with other metals*. Sulphur, by be-

* As sulphur is a nonconductor, and easily excited by slight friction, or small changes in its temperature, some caution is required in drawing conclusions from the experiments in which it is employed. Sulphur, examined immediately after having been heated, gives a positive charge to conductors, agreeing in this respect with the alkaline substances; and a slight contact with the dry hand is sufficient to render it negative. In general likewise in experiments of contact care should be taken that the metallic plate is free from electricity: well polished plates of copper and zinc will, I find, receive a negative charge from being laid on a table of common mahogany.

ing

General principles of the chemical changes produced by electricity.

ing rubbed or struck against newly polished lead, always became positive. Mr. Wilke perhaps was misled by using tarnished lead: sulphur, I find, rubbed against litharge, or lead the surface of which has been long exposed to air, becomes negative; and this exception being removed, all the facts on the subject are confirmations of the general principle*.

On the general principle, oxigen and hidrogen ought to possess, with regard to the metals respectively, the negative and positive energy. This I have not been able to prove by direct experiments of contact; but the idea is confirmed by the agency of their compounds; thus I have found, that solution of sulphuretted hidrogen in water acts in the electrical apparatus composed of single plates and different strata of fluids, in the same manner as alkaline solutions; and that solution of oximuriatic acid is more powerful in similar arrangements than solutions of muriatic acid of a higher degree of concentration; and in both these cases, it is impossible to conceive the combined hidrogen and oxigen inactive. The inference likewise is fully warranted by the case of the solutions of alkaline hidroguretted sulphurets, which, consisting principally of alkali and sulphur together in union with water, exhibit the positive energy with regard to the metals in a very high degree. In the series of experiments on Voltaic arrangements constructed with single plates above-mentioned, I found the solutions of hidroguretted sulphurets in general much more active than alkaline solutions, and particularly active with copper, silver, and lead. And in an experiment that I made on a combination of copper, iron, and hidroguretted sulphuret of potash, in 1802, I found that the positive energy of the hidroguretted sul-

* Concentrated solution of phosphoric acid, I find, is decomposed by Voltaic electricity: the phosphorus combines with the negatively electrified metal, and forms a phosphuret; at least this happened in the two cases that I tried with platina and copper. From all analogy it may be inferred, that the electrical energy of this inflammable substance with regard to metals is the same as that of sulphur. I tried some experiments of contact upon it, but without success. Its slow combustion in the atmosphere it is most likely was the cause of the failure: but even in gases not containing free or loosely combined oxigen, its evaporation would probably interfere.

phuret

phurets with regard to the copper was sufficient to overpower that of the iron; so that the electricity did not circulate from the copper to the iron, and from the iron to the fluid, as in common cases, but from the copper to the hidroguretted sulphuret, and from the hidroguretted sulphuret to the iron.

General principles of the chemical changes produced by electricity.

All these details afford the strongest confirmation of the principle. It may be considered almost as a mere arrangement of facts; and with some extensions it seems capable of being generally applied.

Bodies possessing opposite electrical energies with regard to one and the same body, we might fairly conclude would likewise possess them with regard to each other. This I have found by experiment is the case with lime and oxalic acid. A dry piece of lime, made from a very pure compact secondary limestone, and of such a form as to present a large smooth surface, became positively electrical by repeated contacts with crystals of oxalic acid: and these crystals placed upon the top of a condensing electrometer, and repeatedly touched by the lime, which after each contact was freed from its charge, rendered the gold leaves negatively electrical. The tendency of the mere contacts of the acid and alkali with the metal would be to produce opposite effects to those exhibited, so that their mutual agency must have been very energetic.

It will not certainly be a remote analogy to consider the other acid and alkaline substances generally, and oxigen and hidrogen as possessing similar electrical relations; and in the decompositions and changes presented by the effects of electricity, the different bodies naturally possessed of chemical affinities appear incapable of combining, or of remaining in combination, when placed in a state of electricity different from their natural order. Thus, as we have seen, the acids in the positive part of the circuit separate themselves from alkalis, oxigen from hidrogen, and so on; and metals on the negative side do not unite to oxigen, and acids do not remain in union with their oxides; and in this way the attractive and repellent agencies seem to be communicated from the metallic surfaces throughout the whole of the menstruum.

 VIII.

VIII. *On the relations between the electrical energies of bodies, and their chemical affinities.*

Relations between the electrical energies of bodies and their chemical affinities.

As the chemical attraction between two bodies seems to be destroyed by giving one of them an electrical state different from that which it naturally possesses; that is, by bringing it artificially into a state similar to the other, so it may be increased by exalting its natural energy. Thus, whilst zinc, one of the most oxidable of the metals, is incapable of combining with oxigen when negatively electrified in the circuit, even by a feeble power; silver, one of the least oxidable, easily unites to it when positively electrified; and the same thing might be said of other metals.

Amongst the substances that combine chemically, all those, the electrical energies of which are well known, exhibit opposite states; thus, copper and zinc, gold and quicksilver, sulphur and the metals, the acid and alkaline substances, afford apposite instances; and supposing perfect freedom of motion in their particles or elementary matter, they ought, according to the principles laid down, to attract each other in consequence of their electrical powers. In the present state of our knowledge, it would be useless to attempt to speculate on the remote cause of the electrical energy, or the reason why different bodies, after being brought into contact, should be found differently electrified; its relation to chemical affinity is, however, sufficiently evident. May it not be identical with it, and an essential property of matter?

The coated glass plates of Beccaria strongly adhere to each other when oppositely charged, and retain their charges on being separated. This fact affords a distinct analogy to the subject; different particles in combining must still be supposed to preserve their peculiar states of energy.

In the present early stage of the investigation, it would be improper to place unbounded confidence in this hypothesis; but it seems naturally to arise from the facts, and to coincide with the laws of affinity, so ably developed by modern chemists; and the general application of it may be easily made.

Supposing two bodies, the particles of which are in diffe-

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rent electrical states, and those states sufficiently exalted to give them an attractive force superior to the power of aggregation, a combination would take place which would be more or less intense according as the energies were more or less perfectly balanced; and the change of properties would be correspondently proportional.

Relations between the electrical energies of bodies and their chemical affinities.

This would be the simplest case of chemical union. But different substances have different degrees of the same electrical energy in relation to the same body: thus the different acids and alkalis are possessed of different energies with regard to the same metal; sulphuric acid, for instance, is more powerful with lead than muriatic acid, and solution of potash is more active with tin than solution of soda. Such bodies likewise may be in the same state or repellent with regard to each other, as apparently happens in the cases just mentioned; or they may be neutral; or they may be in opposite or attracting states, which last seems to be the condition of sulphur and alkalis that have the same kind of energy with regard to metals.

When two bodies repellent of each other act upon the same body with different degrees of the same electrical attracting energy, the combination would be determined by the degree; and the substance possessing the weakest energy would be repelled; and this principle would afford an expression of the causes of elective affinity, and the decompositions produced in consequence.

Or where the bodies having different degrees of the same energy, with regard to the third body, had likewise different energies with regard to each other, there might be such a balance of attractive and repellent powers as to produce a triple compound; and by the extension of this reasoning, complicated chemical union may be easily explained.

Numerical illustrations of these notions might be made without difficulty, and they might be applied to all cases of chemical action; but in the present state of the inquiry, a great extension of this hypothetical part of the subject would be premature.

The general idea will, however, afford an easy explanation of the influence of affinity by the masses of the acting substances, as elucidated by the experiments of M. Berthollet;

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Relations between the electrical energies of bodies and their chemical affinities.

for the combined effect of many particles possessing a feeble electrical energy may be conceived equal or even superior to the effect of a few particles possessing a strong electrical energy: and the facts mentioned, page 38, confirm the supposition: for concentrated alkaline lixivia resist the transmission of acids by electricity much more powerfully than weak ones.

Allowing combination to depend upon the balance of the natural electrical energies of bodies, it is easy to conceive that a *measure* may be found of the artificial energies, as to intensity and quantity produced in the common electrical machine, or the Voltaic apparatus, capable of destroying this equilibrium; and such a measure would enable us to make a scale of electrical powers corresponding to degrees of affinity.

In the circuit of the Voltaic apparatus, completed by metallic wires and water, the strength of the opposite electricities diminishes from the points of contact of the wires towards the middle point in the water, which is necessarily neutral. In a body of water of considerable length it probably would not be difficult to assign the places in which the different neutral compounds yielded to, or resisted, decomposition. Sulphate of barytes, in all cases that I tried, required immediate contact with the wire: solution of sulphate of potash exhibited no marks of decomposition with the power of 150, when connected in a circuit of water ten inches in length, at four inches from the positive point; but when placed within two inches, its alkali was slowly repelled and its acid attracted *.

Whenever

* In this experiment, the water was contained in a circular glass basin two inches deep, the communication was made by pieces of amianthus of about the eighth of an inch in breadth. The saline solution filled a half ounce measure, and the distance between the solution and the water, at both points of communication, was a quarter of an inch. I mention these circumstances because the quantity of fluid and the extent of surface materially influence the result in trials of this kind. Water included in glass siphons forms a much less perfect conducting chain than when diffused upon the surface of fibrous nonconducting substances of much smaller volume than the diameter of the siphons. I attempted to employ siphons in some of my first experiments; but the very great inferiority

Whenever bodies brought by artificial means into a high state of opposite electricities are made to restore the equilibrium, heat and light are the common consequences. It is perhaps an additional circumstance in favour of the theory to state, that heat and light are always the result of all intense chemical action. And as in certain forms of the Voltaic battery, where large quantities of electricity of low intensity act, heat is produced without light; so in slow combinations there is an increase of temperature without luminous appearance.

Relations between the electrical energies of bodies and their chemical affinities.

The *effect of heat*, in producing combination, may be easily explained according to these ideas. It not only often gives more freedom of motion to the particles, but in a number of cases it seems to exalt the electrical energies of bodies; glass, the tourmalin, sulphur, all afford familiar instances of this last species of energy.

I heated together an insulated plate of copper and a plate of sulphur, and examined their electricities as their temperature became elevated: these electricites, scarcely sensible at 56° Fahrenheit to the condensing electrometer, became at 100° Fahrenheit capable of affecting the gold leaves without condensation; they increased in a still higher ratio as the sulphur approached towards its point of fusion. At a little above this point, as is well known from the experiments of the Dutch chemists, the two substances rapidly combine, and heat and light are evident.

Similar effects may be conceived to occur in the case of oxigen and hidrogen, which form water, a body apparently neutral in electrical energy to most other substances: and we may reasonably conclude that there is the same exaltation of power, in all cases of combustion. In general, when the different energies are strong and in perfect equilibrium, the combination ought to be quick, the heat and light intense, and the new compound in a neutral state. This would seem to be the case in the instance just quoted; and in the circumstances of the union of the strong alkalis and acids. But where one energy is feeble and the other strong,

inferiority of effect as compared with that of amianthus made me altogether relinquish the use of them.

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Relations between the electrical energies of bodies and their chemical affinities.

all the effects must be less vivid; and the compound, instead of being neutral, ought to exhibit the excess of the stronger energy.

This last idea is confirmed by all the experiments which I have been able to make on the energies of the saline compounds with regard to the metals. Nitrate and sulphate of potash, muriate of lime, oximuriate of potash, though repeatedly touched upon a large surface by plates of copper and zinc, gave no electrical charge to them; subcarbonate of soda and borax, on the contrary, gave a slight negative charge, and alum and superphosphate of lime a feeble positive charge.

Should this principle on further inquiry be found to apply generally, the degree of the electrical energies of bodies, ascertained by means of sensible instruments, will afford new and useful indications of their composition.

IX. *On the mode of action on the pile of Volta, with experimental elucidations.*

Mode of action on Volta's pile, with experimental elucidations.

The great tendency of the attraction of the different chemical agents, by the positive and negative surfaces in the Voltaic apparatus, seems to be to restore the electrical equilibrium. In a Voltaic battery, composed of copper, zinc, and solution of muriate of soda, all circulation of the electricity ceases, the equilibrium is restored if copper be brought in contact with the zinc on both sides: and oxigen and acids, which are attracted by the positively electrified zinc, exert similar agencies to the copper, but probably in a slighter degree, and being capable of combination with the metal, they produce a momentary equilibrium only.

The electrical energies of the metals with regard to each other, or the substances dissolved in the water, in the Voltaic and other analogous instruments, seem to be the causes that disturb the equilibrium, and the chemical changes the causes that tend to restore the equilibrium; and the phenomena most probably depend on their joint agency.

In the Voltaic pile of zinc, copper, and solution of muriate of soda, in what has been called its condition of electrical tension, the communicating plates of copper and zinc are in opposite electrical states. And with regard to electricities of such

such very low intensity, water is an insulating body: every copper plate consequently produces by induction an increase of positive electricity upon the opposite zinc plate; and every zinc plate an increase of negative electricity on the opposite copper plate: and the intensity increases with the number, and the quantity with the extent of the series. Mode of action on Volta's pile, with experimental elucidations.

When a communication is made between the two extreme points, the opposite electricities tend to annihilate each other; and if the fluid medium could be a substance incapable of decomposition, the equilibrium, there is every reason to believe, would be restored, and the motion of the electricity cease. But solution of muriate of soda being composed of two series of elements possessing opposite electrical energies, the oxigen and the acid are attracted by the zinc, and the hidrogen and the alkali by the copper. The balance of power is momentary only; for solution of zinc is formed, and the hidrogen disengaged. The negative energy of the copper and the positive energy of the zinc are consequently again exerted, enfeebled only by the opposing energy of the soda in contact with the copper, and the process of electromotion continues, as long as the chemical changes are capable of being carried on.

This theory in some measure reconciles the hypothetical principles of the action of the pile adopted by its illustrious inventor, with the opinions concerning the chemical origin of Galvanism, supported by the greater number of the British philosophers, and it is confirmed and strengthened by many facts and experiments.

Thus the Voltaic pile of 20 pairs of plates of copper and zinc exhibits no permanent electromotive power when the connecting fluid is water free from air*; for this substance does not readily undergo chemical change, and the equilibrium seems to be capable of being permanently restored through it. Concentrated sulphuric acid, which is a much more perfect conductor, is equally inefficient, for it has little action upon zinc, and is itself decomposed only by a very strong power. Piles, containing as their fluid element ei-

* The experiments proving this fact, and the other analogous facts in this page, may be seen detailed in Nicholson's Journal, 4to, Vol. IV, p. 338 and 394; and Phil. Mag. Vol. X, p. 40.

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Mode of action on Volta's pile, with experimental elucidations.

ther pure water or sulphuric acid, will undoubtedly give single shocks, and this effect is connected with the restoration of the equilibrium disturbed by the energies of the metals; but when their extreme plates are connected there is no exhibition, as in usual cases of electromotion. Water containing loosely combined oxigen is more efficient than water containing common air, as it enables oxide of zinc to be formed more rapidly, and in larger quantities. Neutro-saline solutions, which are at first very active, lose their energy in proportion as their acid arranges itself on the side of the zinc, and their alkali on that of the copper; and I have found the powers of a combination, nearly destroyed from this cause, very much revived, merely by agitating the fluids in the cells and mixing their parts together. Diluted acids, which are themselves easily decomposed, or which assist the decomposition of water, are above all other substances powerful; for they dissolve the zinc, and furnish only a gaseous product to the negative surface, which is immediately disengaged.

There are other experiments connected with very striking results, which offer additional reasons for supposing the decomposition of the chemical menstrua essential to the continued electromotion in the pile.

As when an electrical discharge is produced by means of small metallic surfaces in the Voltaic battery, (the opposite states being exalted) *sensible* heat is the consequence, it occurred to me, that if the decomposition of the chemical agents was essential to the balance of the opposed electricities, the effect, in a saline solution, of this decomposition, and of the transfer of the alkali to the negative side, and of the acid to the positive side, ought, under favourable circumstances, to be connected with an increase of temperature.

I placed the gold cones, which have been so often mentioned, in the circuit of the battery with the power of 100, I filled them with distilled water, and connected them by a piece of moistened asbestus, about an inch in length and $\frac{1}{8}$ of an inch diameter; I provided a small air thermometer capable of being immersed in the gold cones, expecting (if any) only a very slight change of temperature; I introduced a drop

a drop of solution of sulphate of potash into the positive cone: the decomposition instantly began: potash passed rapidly over into the negative cone, heat was immediately sensible; and in less than two minutes the water was in a state of ebullition. Mode of action on Volta's pile, with experimental elucidations.

I tried the same thing with the solution of nitrate of ammonia, and in this instance the heat rose to such an intensity as to evaporate all the water in three or four minutes, with a kind of explosive noise; and at last actual inflammation took place, with the decomposition and dissipation of the greatest part of the salt*.

That the increase of the conducting power of the water by the drop of saline solution had little or nothing to do with the effect, is evident from this circumstance. I introduced a quantity of strong lixivium of potash into the cones, and likewise concentrated sulphuric acid, separately, which are better conductors than solutions of the neutral salts; but there was very little sensible effect.

The same principles will apply to all the varieties of the electrical apparatus, whether containing double or single plates; and if the ideas developed in the preceding sections be correct, one property operating under different modifications is the universal cause of their activity.

X. *On some general Illustrations and Applications of the foregoing Facts and Principles, and Conclusion.*

The general ideas advanced in the preceding pages are evidently directly in contradiction to the opinion advanced by Fabroni, and which, in the early stage of the investigation, appeared extremely probable, namely, that chemical changes are the *primary* causes of the phenomena of Galvanism. General illustrations and applications.

Before the experiments of M. Volta on the electricity excited by the mere contact of metals were published, I had to a certain extent adopted this opinion; but the new facts im-

* In this process ammonia was rapidly given off from the surface of the negative cone, and nitrous acid from that of the positive cone, and a white vapour was produced by their combination in the atmosphere above the apparatus.

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General illustrations and applications.

mediately proved, that another power must necessarily be concerned; for it was not possible to refer the electricity exhibited by the opposition of metallic surfaces to any chemical alterations, particularly as the effect is more distinct in a dry atmosphere, in which even the most oxidable metals do not change, than in a moist one, in which many metals undergo chemical alteration.

Other facts likewise soon occurred demonstrative of the same thing. In the Voltaic combination of diluted nitrous acid, zinc, and copper, as is well known, the side of the zinc exposed to the acid is positive. But in combinations of zinc, water, and diluted nitric acid, the surface exposed to the acid is negative; though if the *chemical* action of the acid on the zinc had been the cause of the effect, it ought to be the same in both cases.

In mere cases of chemical change likewise electricity is never exhibited. Iron burnt in oxigen gas, properly connected with a condensing electrometer, gives no charge to it during the process. Nitre and charcoal deflagrated in communication with the same instrument do not by their agencies in the slightest degree affect the gold leaves. Solid pure potash and sulphuric acid made to combine in an insulated platina crucible produce no electrical appearances. A solid amalgam of bismuth and a solid amalgam of lead become fluid when mixed together: the experiment, I find, is connected with a diminution of temperature, but with no exhibition of electrical effects. A thin plate of zinc, after being placed upon a surface of mercury, and separated by an insulating body, is found positive, the mercury is negative: the effects are exalted by heating the metals; but let them be kept in contact sufficiently long to amalgamate, and the compound gives no signs of electricity. I could mention a great number of other instances of *pure chemical* action in which I have used all the means in my power to ascertain the fact, and the result has been constantly the same. In cases of effervescence, indeed, particularly when accompanied by much heat, the metallic vessels employed become negative, but this is a phenomenon connected with *evaporation*, the change of state of a body

body independent of chemical change, and is to be referred to a different law*.

General illustrations and applications.

I mentioned the glass plates of Beccaria as affording a parallel to the case of combination in consequence of the different electrical states of bodies. In Guyton de Morveau's experiments on cohesion, the different metals are said to have adhered to mercury with a force proportional to their chemical affinities. But the other metals have different electrical energies, or different degrees of the same electrical energy with regard to this body; and in all cases of contact of mercury with another metal, upon a large surface, they ought to adhere in consequence of the difference of their electrical states, and that with a force proportional to the exaltation of those states. Iron, which M. Guyton found slightly adhesive, I find exhibits little positive electricity after being laid upon a surface of mercury, and then separated. Tin, zinc, and copper, which adhere much more strongly, communicate higher charges to the condensing electrometer: I have had no instrument sufficiently exact to measure the differences: but it would seem, that the adhesion from the difference of electrical states must have operated in these experiments†, which being proportional to the electrical energies are, on the

* The change of the capacities of bodies in consequence of the alteration in their volumes, or states of existence by heat, is a continually operating source of electrical effects: and as I have hinted, page 47, it often interferes with the results of experiments on the electrical energies of bodies as exhibited by contact. It is likewise probably one of the sources of the capricious results of experiments of friction, in which the same body, according as its texture is altered, or its temperature changed, assumes different states with regard to another body. Friction may be considered as a succession of contacts, and the natural energies of bodies would probably be accurately exhibited by it, if the unequal excitation of heat or its unequal communication to the different surfaces did not interfere by altering unequally their electrical capacities. Of the elements of flint glass, silex is slightly negative with regard to the metals, the soda is positive; and in contacts of glass with metals I find it exhibits the excess of the energy of the alkali: the case, as is well known, is the same in friction, the amalgam of the common machine is essential to its powerful excitation.

† Amalgamation undoubtedly must have interfered; but the *general* result seems to have been distinct.

hypothesis

General illustrations and applications.

hypothesis before stated, proportional to the chemical affinities. How far cohesion in general may be influenced or occasioned by this effect of the difference of the electrical energies of bodies is a curious question for investigation.

Many applications of the general facts and principles to the processes of chemistry, both in art and in nature, will readily suggest themselves to the philosophical inquirer.

They offer very easy methods of separating acid and alkaline matter, when they exist in combination, either together or separately, in minerals; and the electrical powers of decomposition may be easily employed in animal and vegetable analysis.

A piece of muscular fibre, of two inches long and half an inch in diameter, after being electrified by the power of 150 for five days, became perfectly dry and hard, and left on incineration no saline matter. Potash, soda, ammonia, lime, and oxide of iron were evolved from it on the negative side, and the three common mineral acids and the phosphoric acid were given out on the positive side.

A laurel leaf, treated in the same manner, appeared as if it had been exposed to a heat of 500° or 600° Fahrenheit, and was brown and parched. Green colouring matter, with resin, alkali, and lime, appeared in the negative vessel: and the positive vessel contained a clear fluid, which had the smell of peach blossoms; and which, when neutralized by potash, gave a blue-green precipitate to solution of sulphate of iron; so that it contained vegetable prussic acid.

A small plant of mint, in a state of healthy vegetation, was made the medium of connection in the battery, its extremities being in contact with pure water; the process was carried on for 10 minutes: potash and lime were found in the negatively electrified water, and acid matter in the positively electrified water, which occasioned a precipitate in solutions of muriate of barytes, nitrate of silver, and muriate of lime. This plant recovered after the process: but a similar one, that had been electrified for four hours with like results, faded and died*.

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* Seeds, I find, when placed in pure water in the positive part of the circuit, germinate much more rapidly than under common circumstances; but

The facts show, that the electrical powers of decomposition act even upon living vegetable matter; and there are some phenomena which seem to prove, that they operate likewise upon living animal systems. When the fingers, after having been carefully washed with pure water, are brought in contact with this fluid in the positive part of the circuit, acid matter is rapidly developed, having the characters of a mixture of muriatic, phosphoric, and sulphuric acids: and if a similar trial be made in the negative part, fixed alkaline matter is as quickly exhibited. General illustrations and applications.

The acid and alkaline tastes produced upon the tongue, in Galvanic experiments, seem to depend upon the decomposition of the saline matter contained in the living animal substance, and perhaps in the saliva.

As acid and alkaline substances are capable of being separated from their combinations in living systems by electrical powers, there is every reason to believe, that by converse methods they may be likewise introduced into the animal economy, or made to pass through the animal organs: and the same thing may be supposed of metallic oxides; and these ideas ought to lead to some new investigations in medicine and physiology.

It is not improbable, that the electrical decomposition of the neutral salts in different cases may admit of economical uses. Well burned charcoal and plumbago, or charcoal and iron, might be made the exciting powers; and such an arrangement, if erected upon an extensive scale, neutrosaline matter being employed in every series, would, there is every reason to believe, produce large quantities of acids and alkalies with very little trouble or expense.

Ammonia, and acids capable of decomposition, undergo chemical change in the Voltaic circuit only when they are in very concentrated solution, and in other cases are merely carried to their particular points of rest. This fact may induce

but in the negative part of the circuit they do not germinate at all. Without supposing any peculiar effects from the different electricities, which however may operate, the phenomenon may be accounted for from the saturation of the water near the positive metallic surface with oxigen, and of that near the negative surface with hidrogen.

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General illustrations and applications.

us to hope, that the new mode of analysis may lead us to the discovery of the *true* elements of bodies, if the materials acted on be employed in a certain state of concentration, and the electricity be sufficiently exalted. For if chemical union be of the nature which I have ventured to suppose, however strong the natural electrical energies of the elements of bodies may be, yet there is every probability of a limit to their strength: whereas the powers of our artificial instruments seem capable of indefinite increase.

Alterations of electrical equilibrium are continually taking place in nature; and it is probable that this influence, in its faculties of decomposition and transference, considerably interferes with the chemical alterations occurring in different parts of our system.

The electrical appearances which precede earthquakes and volcanic eruptions, and which have been described by the greater number of observers of these awful events, admit of very easy explanation on the principles that have been stated.

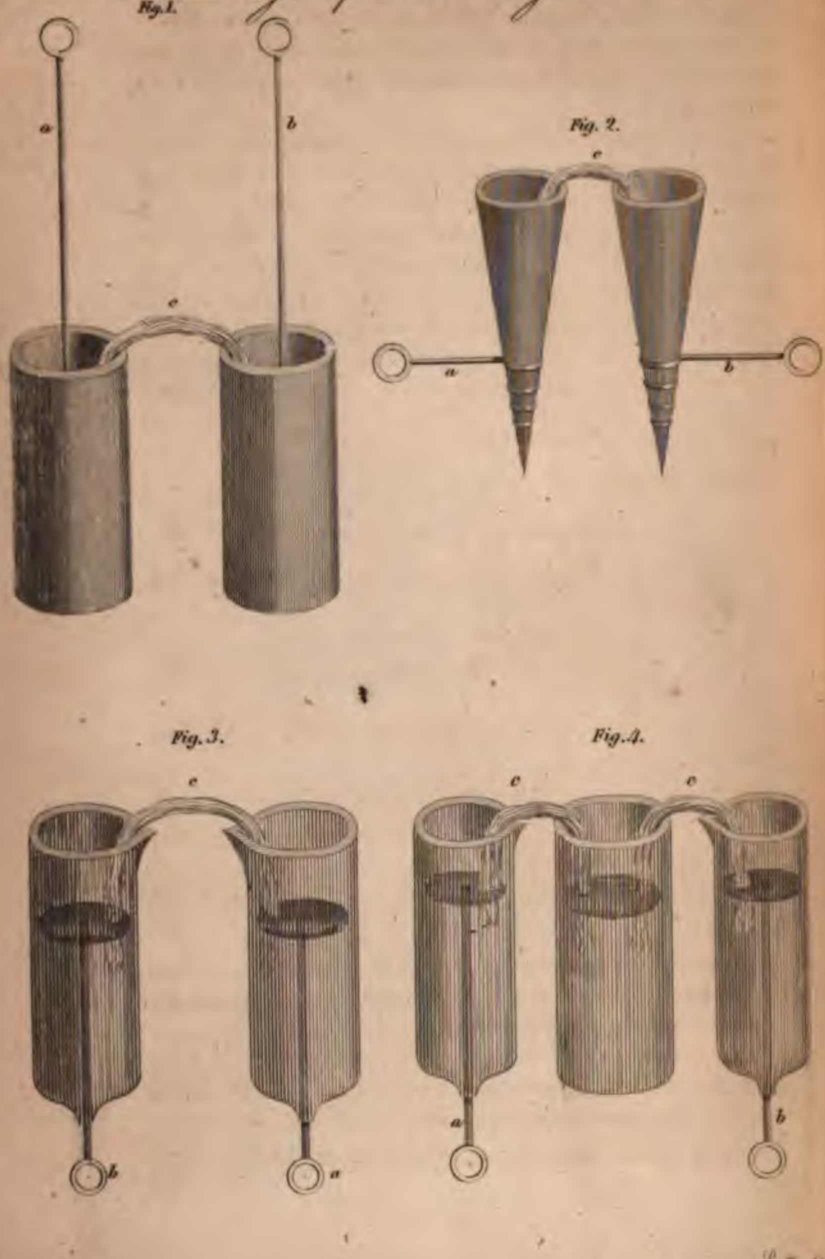
Beside the cases of sudden and violent change, there must be constant and tranquil alterations, in which electricity is concerned, produced in various parts of the interior strata of our globe.

Where pyritous strata and strata of coal-blende occur, where the pure metals or the sulphurets are found in contact with each other, or any conducting substances, and where different strata contain different saline menstrua, electricity must be continually manifested; and it is very probable, that many mineral formations have been materially influenced, or even occasioned by its agencies.

In an experiment that I made of electrifying a mixed solution of muriates of iron, of copper, of tin, and of cobalt, in a positive vessel, distilled water being in a negative vessel, all the four oxides passed along the asbestus, and into the negative tube, and a yellow metallic crust formed on the wire, and the oxides arranged themselves in a mixed state round the base of it.

In another experiment, in which carbonate of copper was diffused through water in a state of minute division, and a negative wire placed in a small perforated cube of zeolite in the

Mr. Davy's Experiments on Galvanism.



the water, green crystals collected round the cube; the particles not being capable of penetrating it. General illustrations and applications.

By a multiplication of such instances the electrical power of transference may be easily conceived to apply to the explanation of some of the principal and most mysterious facts in geology.

And by imagining a scale of feeble powers, it would be easy to account for the association of the insoluble metallic and earthy compounds containing acids.

Natural electricity has hitherto been little investigated, except in the case of its evident and powerful concentration in the atmosphere.

Its slow and silent operations in every part of the surface will probably be found more immediately and importantly connected with the order and economy of nature; and investigations on this subject can hardly fail to enlighten our philosophical systems of the Earth; and may possibly place new powers within our reach.

Explanation of the Figures.

Pl. I. Fig. 1, Represents the agate cups, mentioned Vol. XVIII, p. 323.

Fig. 2, Represents the gold cones, page 325.

Fig. 3, Represents the glass tubes, and their attached apparatus, page 337.

Fig. 4, Represents the two glass tubes, with the intermediate vessel, page 338.

In all the figures A B denote the wires, rendered one positively, the other negatively electrical; and C the connecting pieces of moistened amianthus.

XIII.

Memoir on the Analysis of the Sweat, the Acid it contains, and the Acids of the Urine and Milk; read to the National Institute by Mr. THENARD*.

IF we examine the principal fluids of the animal economy, we find, that some are alkaline, and the others acid. To the first Animal fluids acid or alkaline.

* Annales de Chimie, vol. LIX, p. 262, Sept. 1806.

first class belong the blood and bile: to the second, the urine, milk, and sweat.

Hence arise naturally two questions; what are the alkalis, and what are the acids, proper to these fluids? The first has already been solved, as the researches of Cadet and Deyeux have proved, that we never meet with any alkali but soda in animal substances. The solution of the second however is but little advanced: even the data, that might lead to it, are for the most part inaccurate: and many of the results relating to some of these parts are too deficient in proof, to be placed in the rank of demonstrated truths. This is the question therefore, that will form the subject of the present memoir; and, that I may treat it in a manner suitable to the object I have in view, I shall first present as full an analysis of the sweat, as we have of urine and of milk.

Soda the only alkali.

What are the acids.

Part I. *Of the Sweat.*

Sweat.

The sweat is a fluid separated from the blood in the skin by exhalant vessels, with which its texture is traversed or filled. It is more or less copious in different individuals: and its quantity is perceptibly in the inverse ratio of that of the urine. All other circumstances being similar, much more is produced during digestion than during repose. The maximum of its production appears to be twenty-six grains and two thirds in a minute, the minimum nine grains, troy weight. It is much inferior however to the pulmonary transpiration: and there is likewise a great difference between their nature and manner of formation. The one is the product of a particular secretion, similar in some sort to that of the urine: the other, composed of a great deal of water and carbonic acid, is the product of a combustion gradually effected by the atmospheric air.

That of an adult from 1320 grs. near 2 lbs. avoird., to 38400, near 5½ lbs. per day.

That from the lungs still more.

The first a secretion.

Its qualities.

The sweat, in a healthy state, very sensibly reddens litmus, paper or infusion. In certain diseases, and particularly in putrid fevers, it is alkaline: yet its taste is always rather saline, and similar to that of salt, than acid. Though colourless, it stains linen. Its smell is peculiar, and insupportable when it is concentrated, which is the case in particular during distillation. But before I speak of the trials to which I subjected it, and for which I had occasion for a great quantity,

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I ought to mention the method I adopted for procuring it.

I applied to persons who are in the habit of wearing flannel waistcoats next the skin. To avoid every source of errour, the waistcoats, before they were put on, were first washed with soap; then rinsed in a stream of water, and afterward in diluted muriatic acid several times; and lastly they were immersed and wrung out of a large tub of water. The persons who were so obliging as to submit to the experiment, went into the bath before they began it, and were particularly careful to rub every part of the body well. The sweat that was collected uninterruptedly in the flannel during the course of ten days I separated by means of hot distilled water; and this I boiled down to the consistence of a sirup in a retort, to the neck of which a receiver was adapted. The product of this distillation emitted a nauseous smell, which diminished as the liquor cooled. It caused no alteration in sirup of violets, but it evidently reddened infusion of litmus. Left for some time exposed to the air, it retained the transparency it had at first, and underwent no remarkable change, unless with respect to its smell, which entirely vanished: in a close vessel probably it would have putrified, like the product of the distillation of all other animal fluids.

How obtained by the author.

Distilled.

The residuum was not very copious, and evidently void of smell; though pretty strongly acid, the agreeable taste of sea salt predominated in it, yet with this taste something acrid and pungent was perceptible; it was slightly deliquescent, requiring some days to resolve into a liquid; and it was completely soluble in water. Lime, barytes, ammonia, the acidulous oxalate of potash, the carbonates of potash and soda, most acids, and acetate of lead, gave no precipitate with this solution, and disengaged nothing from it. Nutgalls occasioned a slight precipitate in it, but the nitrate of silver rendered it very turbid.

Residuum.

Calcined by itself it was decomposed, emitting vapours that had nothing of the fetid smell of animal matter, and was converted into a black substance, that was composed simply of a great deal of common salt, charcoal, and scarcely perceptible quantities of lime and oxide of iron.

Calcined.

Finally, when subjected to calcination after the acid has been

Calcined after

saturation with potash. been saturated with potash, this base was obtained in the state of carbonate, beside the preceding matters, in the black substance remaining.

Contains common salt, very little phosphate of lime, oxide of iron, & animal matter, and an acid. These trials already convinced me, that sweat contains muriate of soda, traces of phosphate of lime and oxide of iron, very little animal matter, no sulphate, no soluble phosphate, and in addition an acid, the nature of which I already suspected.

This probably the acetous. In fact this acid, combined with a base, giving rise to a carbonate by its calcination, must belong to the vegetable or animal kingdom; and as besides it was volatile, and formed soluble salts with the different salifiable bases, it became very probable that it was the acetous acid.

Yet it might be a new acid. Led by this reasoning to suppose the existence of acetous acid in sweat, I still required possitive experiments, to convince myself of it: for though the properties I have mentioned belong only to the acetous, of all the known acids, yet they might equally belong to an unknown acid. Positive proof to be sought where practicable. Thus azote is far from being sufficiently characterised by the properties with which we usually content ourselves as denoting its presence; namely, its being without smell, without colour, and without action on blue colours or solution of lime; all negative properties, and far from being as characteristic as those, which, being founded on combinations, may be termed positive. Farther, to give certainty, there must be a combination of these positive properties, unless some one, which happens in certain instances, be so decisive, as to suffice of itself.

The acid obtained separate. Thus, though every thing apparently tended to show me, that the acid of sweat was the acetous, it was neceffary for me to obtain it separate, and combine it with different substances, before I would pronounce definitively on its nature. This I effected easily, by distilling with another acid the residuum, which a certain quantity of sweat collected in a flannel waistcoat slightly alkaline afforded by evaporation. In this distillation I preferred the phosphoric acid; on one hand, because it is fixed; and on the other, because as it is very difficult to decompose, it acts less on organic matters than many others. I farther took every precaution, to condense the product of distillation in the receiver. Its properties. This product strongly reddened

reddened infusion of litmus: its taste was that of a weak acid: its smell that of vinegar: combined with potash it formed a salt, which by evaporation was reduced to little shining scales, micaceous as it were, acrid, and very deliquescent: on the addition of sulphuric or phosphoric acid this salt evolved a strong smell of acetic acid; and, poured into a solution of nitrate of mercury, it precipitated crystalline scales, similar to accetite of mercury.

This acid therefore was the acetous, and consequently human sweat is formed of a great deal of water: free acetous acid; muriate of soda; an atom of phosphate of lime and oxide of iron; and an inappreciable quantity of animal matter, which approaches much nearer to gelatine than to any other substance. This acid the acetous. The animal matter resembled gelatine.

Part. II. *Of the acids of urine.*

These acids are, 1st, the uric acid, which frequently gives rise to the stone in the bladder: 2dly, the benzoic acid, which exists very rarely in that of adults or old persons, and is more frequent in that of infants: 3dly, we are obliged to admit another acid, since the urine strongly and constantly reddens tincture of litmus, an action which cannot be ascribed either to the uric acid, that does not alter its colour, or to the benzoic acid, that is found in the urine only under certain circumstances, which are not yet well known. Urine contains lithic acid, sometimes benzoic, and a third.

What is this new acid? This is the second question that I shall attempt to discuss. At present it is generally supposed to be the phosphoric acid. This opinion is grounded on the presence of a pretty large quantity of phosphate of lime in urine, which, being itself insoluble when neutral, becomes very soluble and even deliquescent, when it is with an excess of acid: and at the same time it is strengthened by the consideration, that beside the phosphates of lime, soda, ammonia, and magnesia, we find nothing in urine but the sulphates of potash and soda, and muriates of soda and ammonia, neither of which salts is decomposed by the acidulous phosphate of lime: their acids therefore, that is the sulphuric and muriatic, cannot exist in the urine, since, as is well known, they would convert the phosphate of lime into acidulous phosphate of lime. If then the phosphoric acid be What is this acid? Supposed to be phosphoric.

not the solvent of the phosphate of lime in urine, it must undoubtedly be some other weak acid, and probably an acid of the nature of the vegetable and animal acids.

This probably a mistake.

Nothing in fact proves, that this is not the case. I will venture to say farther, that this hypothesis appears to me more admissible than the former: for, to admit the acidulous phosphate of lime in urine, we must suppose, that a portion of one of the phosphates of the blood is decomposed in the kidneys, when it reaches them: that the phosphoric acid is free, or at least constitutes an acidulous phosphate with the phosphate of lime, though present with the soda of the blood, and with the base of the phosphate decomposed, both of which appear not to enter into any new combination at the time, and which are taken up with the residuum of the secretion by the venous system, to be returned into the circulation.

In the living body chemical action may be restrained.

It is true it may be said, that bodies under the influence of life act in a different manner from what they do when deprived of it; and that consequently decompositions may take place in the animal economy contrary to all that we are acquainted with. But, beside that this answer, though accurate, proves little in favour of the case in question, it may be employed in a certain degree to retort the argument, as thus: we have no avowed instance of salts being decomposed in the animal economy so that their alkali and acid remain present together without combining, while on the other hand it is demonstrated, that animal substances, particularly those that exist in the blood, as the fibrine and albumen, are transformed into some other in passing through this or that organ; thus in the mammary glands they are converted into sugar of milk, and the caseous, butyraceous, and extractive matters; and in the kidneys they form uree, uric acid, and sometimes benzoic acid. Now if they constantly form one of these acids, and sometimes the other likewise, it is possible they may form a third, which combines with the phosphate of lime, and holds it in solution. Such were the reflections that have led me to examine the acid of urine; and I shall proceed to relate the experiments, that I have made to discover its nature.

After having employed several means, which I shall pass over,

over, as they were without success, at least directly, I evaporated almost to dryness, in a water-bath, that I might not decompose the uree, about twenty quarts of fresh urine. The residuum powerfully reddened infusion of litmus; and I treated it cold, at several times, with a great deal of alcohol at 36° of strength.

Urine evaporated continued an acid.

I thus dissolved the greater part of the acid; but I could not effect its complete solution, whatever quantity of alcohol I employed, and even by the assistance of a small degree of heat. Having mixed all the liquors, I concentrated them by evaporation at a low temperature. I then examined the matter, which I had afresh reduced to a sirupy consistence. First I diluted a portion with water, and added to it lime-water and ammonia. No precipitate took place, or at least it was so slight, that it did not appear till long after the mixture was made. Another portion I calcined. The residuum was not only not acid; but, even treated with water, the calcareous salts and lime-water, added to the solution, gave no indication of an atom of phosphate. That which was not dissolved, and which contained a great deal of coal when completely incinerated, merely left a few traces of phosphate of lime.

This separated in great part.

Examined.

Hence it should seem, that urine contains, beside the uric acid, an acid with at least a binary radical. I strongly suspected, that it was the acetous; because I had already found this acid in other animal fluids, it exists in almost all vegetables, and it is formed in almost all the decompositions, that organized bodies undergo. In consequence into the portion I had left containing the acid I poured barytes-water. Having then evaporated the mixture to dryness, still with a gentle heat, I treated it afresh with alcohol, which dissolved the whole, except a yellowish powder, that was true acetate of barytes. Thus from this experiment we may infer, that there is acetous acid in urine; though it does not prove, that there is no phosphoric acid, since urine evaporated by a water-bath, and treated with a great deal of alcohol, always leaves a slightly acid residuum, and this acid, it may be said, is the phosphoric.

It has at least a binary radical.

Barytes added,

formed acetate of barytes.

To demonstrate, that this acid is not really the phosphoric, I could not have recourse to calcination; for the residuum, containing phosphate of ammonia, could not have failed

Attempt to prove, that it contains no

free phosphoric acid.

failed to yield phosphoric acid: I was under the necessity therefore of adopting the synthetical method. Accordingly after having saturated by means of potash the extract of some urine, that I had evaporated to dryness with the precautions already described, I poured in a little vinegar, treated it with alcohol, and obtained the same results as I have already related; that is to say, the portion, that was not dissolved after repeated affusions of alcohol, was acid. This proof, I am aware, may still be questioned: for, if the phosphoric acid existed in the urine, it would be partly retained by the salts present in it, in the same manner as the acetous, and would become insoluble in alcohol. But if it be considered, that the existence of the acetous acid in urine appears certain*; that nothing demonstrates the presence of the phosphoric; that the greater part of the free acid of the urine evaporated to the consistence of a sirup dissolves in alcohol; and that all this acid, thus dissolved, is the acetous: lastly, if we recollect, that the residuum is slightly acid; and that, if saturated with potash, afterward acidulated with vinegar, and treated afresh with alcohol, it remains equally acid: all these circumstances compared together, I conceive, will acquire such a degree of certainty, as absolutely to convince us, that it is the acetous acid alone in urine which dissolves the phosphate of lime, and which alone too most commonly imparts to it the property of reddening infusion of litmus.

Farther proof that it is the acetous only.

But, to render this last conclusion still more evident, I ought to demonstrate, more directly than has hitherto been done, that the benzoic acid is not in fact a constant principle of urine. For this, instead of employing sublimation with or without an excess of another acid, when the urine is reduced to a sirupy consistence; a method always inaccurate, since the benzoic acid combined with ammonia is car-

* I believe, that, in the evaporation of the urine in a water bath, a little uree is decomposed, and that ammonia, and perhaps a little acetous acid is formed Supposing this to be the case, it still remains very probable, that the acid of urine is the acetous acid, and not any other: for in favour of this opinion I might not only adduce the reasons that have been, or that will be given, but even the tendency the uree would have in this case to be converted into acetous acid.

ried

ried off more or less with the water that rises in vapour; I added lime before I began the evaporation, and treated the extract with alcohol.

It is true by this method we dissolve, beside the benzoate of lime, some uree, muriate of ammonia, and soda, and acetous acid: but if the alcoholic solution be converted into a concentrated aqueous solution, the acids added afterward will soon manifest the presence of benzoic acid, if there be ever so little in the solution.

Mode of analysing urine.

Thus, when we would analyse urine, the benzoic acid should be first sought for, either by this or some analogous process. If by this we discover no trace of it in the liquid, which is most commonly the case, we may conclude, that it does not contain any sensible quantity of it: then, after having evaporated another portion of the urine in a water-bath, and thus ascertained the quantity of water that enters into its composition, the residuum must be treated repeatedly with alcohol at 36°: thus we shall dissolve the uree, the muriate of ammonia, some muriate of soda, and the greater part of the acetous acid.

The mixture of these different substances should be divided into three portions. From the first the acetous acid is to be separated by the means pointed out. From the second the uree is to be extracted by concentrated nitric acid, from which again it is to be separated by the carbonate of potash and alcohol *. Lastly, from the third part the quantity of sal ammoniac and muriate of soda is to be ascertained by sublimation. In this sublimation the uree is destroyed, the acetous acid is volatilized, the muriate of soda remains behind, and is to be weighed: the sal ammoniac sublimes, and is to be collected; and as it is always mixed with black matters, and may besides contain a little carbonate of ammonia, it is to be purified by dissolving it in water and evaporating the solution.

Soluble mat-

The matters contained in urine, that are soluble in alco-

Uree does not crystallize without the addition of some salt.

* Pure uree does not crystallize: it is only when combined with certain salts, which frequently happens, that it forms crystals. I believe, but I am not certain, that it renders several salts soluble in alcohol, which when alone are insoluble in it. This might easily be verified with muriate of barytes.

hol,

ters contained in urine.

hol, are five; namely, acetous acid, benzoic acid, muriate of ammonia, muriate of soda in part, and uree. Those that are insoluble in it are more numerous, as at least eight may be reckoned; namely, four phosphates, two sulphates, muriate of soda, and uric acid. On treating with water these eight substances insoluble in alcohol, we dissolve the phosphates of soda and ammonia, a very little phosphate of magnesia, the muriate of soda, the sulphates of potash and soda, which are known by their crystallization, and which may be separated from one another in a certain degree by solutions of platina. We may judge that phosphate of magnesia is present by means of potash, which will precipitate a small quantity of this earth.

The substances insoluble in water then are the phosphate of lime, some phosphate of magnesia combined with phosphate of ammonia, and uric acid, which may be separated in the usual way. This method however differs very little from those that have been given by other chemists; and I describe it here in a concise manner, because it is intimately connected with my subject.

Part III. *Of the acid of milk.*

Milk quite fresh contains a free acid.

Milk as soon as it comes from the mammary glands reddens litmus paper: it contains therefore a free acid. When I discovered this fact near eighteen months ago, I endeavoured in vain to obtain it pure, in order to examine its properties: and all my endeavours since that time, to attain the same object, have been equally fruitless.

Probably the acetous.

Though every thing leads us to believe, that it is the acetous acid, yet it is the same with respect to it, as with respect to the acids of sweat and urine: to pronounce decidedly on its nature, it was necessary to separate it, and combine it afterward with salifiable bases. This at length I effected, by pursuing a method analogous to that, which enabled me to obtain the acid of urine. 1st, I evaporated the milk to dryness: 2dly, I treated the residuum with barytes water, to saturate the acid: 3dly, I evaporated to dryness again: 4thly, I treated it with alcohol, to dissolve in part the extractive matter, and particularly to collect the caseous substance, so that none should remain suspended in the water:

This proved.

5thly,

5thly, I macerated in water what was not dissolved by the alcohol, filtered the liquor, concentrated it by evaporation, and distilled it with phosphoric acid. By these means I collected in the receiver a fluid, which possessed all the properties of acetous acid.

It follows then, from the various experiments I have described, 1st, that urine probably contains no free phosphoric acid: but that there is to be found in it, as well as in the milk and sweat, acetous acid. 2dly, That the sweat contains, beside this, a great deal of water, some muriate of soda, a small quantity of animal matter, and some traces of oxide of iron and phosphate of lime. General conclusions.

It is probable, that the acetous acid exists in several other substances. Several observations lead me to believe, that it would be found in cantharides: the analogy of the bombic and formic acids with vinegar have already been suspected: and I would almost venture, to generalize this idea, and say, that it exists in almost all animals, as in the sap of almost all vegetables: at least we may affirm, that of all the acids its formation costs nature least; its principles having such a tendency to unite, that we can scarcely ever disturb the equilibrium of the molecules of organized substances, without producing more or less of it. If the decomposition be rapid, acetous acid is formed; if slow, it is formed still: witness the distillation of vegetable and animal substances, their treatment by nitric and by oxigenized muriatic acid, their spontaneous decomposition, and their transformation into vegetable mould or adipocire. Acetous acid probably exists in several other substances, perhaps in most animals. Most easily formed.

In cases of indigestion it is known, that the food becomes acid, and this too is owing to acetous acid. In several circumstances however, its production has not yet been thoroughly appreciated: it remains to be seen, whether it exist in the milk of all kinds of animals; whether it be found in the sweat of all, and whether the sweat of different animals be identical; and lastly, whether it be not in the state of acetate in such urine as is alkaline. This is an inquiry which I propose to undertake, and the results of which I shall submit to the judgment of the Institute, if they prove worthy its attention. Formed in indigestion. Farther inquiries intended.

XIV.

XIV.

Remarks on Orpiment and Realgar: by Mr. THENARD*.

Orpiment and realgar,

ORPIMENT and realgar are two ores of arsenic sufficiently abundant. The first is almost always in the form of laminæ of a pure yellow colour; and the second is as generally a red mass more or less brown. Bucquet asserted, that these compounds were formed of oxide of arsenic, and sulphur, in the same proportions, and ascribed their difference of colour to the different degree of heat employed in preparing them. Bergman likewise admitted the oxide of arsenic, as well as sulphur, in both; but he imagined they differed in colour because they contained different proportions. These opinions, supported by some experiments that were capable of deceiving, prevented chemists for some time from forming a decided opinion: that of the Swedish chemist however prevailed, and since the creation of the new theory, and the reform of chemical language, orpiment and realgar are described in chemical treatises under the names of yellow sulphuret of oxide of arsenic, and red sulphuretted oxide of arsenic. Nevertheless some have lately thought, that these two substances differed less with respect to their proportions of sulphur, than those of their oxigen.

said to be the same sulphuret of arsenic modified by heat; then sulphuretted oxides, differing in their proportions; and lastly sulphurets of different oxides.

Thus it has been successively supposed, 1st, that orpiment and realgar were homogeneal compounds containing burned arsenic: 2dly, that they were oxides more or less sulphuretted: and 3dly, that they were oxides more or less oxided, as well as more or less sulphuretted.

Arguments for the first opinion.

The partisans of the first opinion ground it on the fact, that by heating equal quantities of arsenious acid and sulphur in a less or greater degree the product is sometimes orpiment, at others realgar: therefore say they, if their colour differ, it is owing to the heat, which occasions a different arrangement of their particles.

* Annales de Chimie, vol. LIX, p. 284, Sept. 1806. This paper was read to the Philomathic Society about a year ago.

Those

Those of the second refer to the analysis of orpiment and realgar in the humid way. As they obtained from the latter much more oxide of arsenic, and less sulphur, than from the former, their conclusion appeared to them just. For the second.

Those of the third argue from analogy. They imagine, that, when a metallic solution is precipitated by a hidrosulphuret, the sulphuretted oxide that is formed is always of the colour of the oxide it contained. For the third.

It is easy to perceive, that none of these reasonings are free from objection: and hence I have imagined it would not be useless, to subject both orpiment and realgar to a fresh examination, in order to find with precision how they differ from each other. All liable to objection.

But before I speak of the experiments however, which I have made with them, I ought to quote what prof. Proust says of orpiment in the Journal de Physique, vol. XLIX, pp. 411, 412: particularly as I am perfectly of his opinion respecting the nature of this compound. Proust says, that, in preparing orpiment,

"Things happen differently," says Mr. Proust, "when, instead of applying potash to the sulphuret of antimony, we add it to the ore of arsenic: the sulphuretted hidrogen, that is formed while the arsenic becomes oxided, does not adhere to this oxide, on precipitating it with an acid, as happens to that of antimony. The hidrogen acts a very different part during this precipitation: it is employed in disoxiding the arsenic, in order that it may attach itself as a metal to the sulphur, and produce the yellow sulphuret, which we call orpiment: for the hidrosulphuret of arsenic, and the sulphuretted oxide, are two combinations that apparently do not exist. If we dissolve white arsenic in thoroughly saturated hidrosulphuret of potash, and afterward add an acid, orpiment is precipitated without the least disengagement of gas, without the slightest smell: but on the one hand the sulphuretted hidrogen is no longer to be found, and on the other the arsenic in the orpiment is in the metallic state: in this precipitation therefore water is formed. The pure regulus of arsenic is not soluble in the arsenical hidrosulphuret." the oxide of arsnic is decomposed by the hidrogen, and the arsenic unites in the metallic state with the sulphur.

If I might be permitted to make one observation on this passage in Mr. Proust's paper, I would say, that, it seems His experiments scarcely

to

prove the absence of oxigen.

to me, the experiments adduced by this learned chemist are not altogether sufficient to prove the nonexistence of oxigen in orpiment: for we may account for the result, whether we admit the existence of sulphuretted hidrogen in this compound, or that of an oxide less oxided than the white oxide of arsenic. Mr. Proust has said nothing of realgar.

Both sulphurets of arsenic decomposed on the open fire, and sublimed in close vessels.

Both orpiment and realgar, if reduced to powder, and projected on burning coals, melt, swell up, and emit sulphurous acid: but all these phenomena are more obvious with realgar. Heated in close vessels the fusion and tumefaction are the same, and they are sublimed without changing their nature, consequently without giving out any sulphurous acid.

Realgar contains most arsenic.

Sulphur fused with realgar converts it into orpiment; while arsenic fused with orpiment converts it into realgar.

Acids that attack them.

The sulphuric, nitric, nitrous, and oxigenized muriatic acid, are, as is well known, the only ones that attack orpiment and realgar.

Sulphuric.

Sulphuric acid acts perceptibly with greater power on orpiment than on realgar. In both cases sulphurous acid is formed, and likewise arsenious acid; but more sulphurous acid, and less arsenious, are produced with the orpiment.

Nitric.

Nitric acid is decomposed by both these substances, even without the assistance of heat: and orpiment affords with it more sulphur, and less arsenious acid, than realgar.

Oxigenized muriatic and nitro-muriatic.

With oxigenized muriatic acid, and with the nitro-muriatic, the same results are obtained as with the nitric.

Alkalis.

The alkalis, particularly potash and soda, easily dissolve both, even cold. Hidroguretted sulphuret of potash and arsenite of potash are formed: since on pouring lime-water into the solution a pretty copious white precipitate is obtained, which, treated with carbonate of potash, affords a liquor, that yields, on adding a sufficient quantity of muriatic acid, and evaporating to a proper point, a great deal of arsenious acid.

Orpiment contains most sulphur, and neither probably any oxigen.

All these experiments show, that more sulphur is contained in orpiment than in realgar, and some of them lead us to suspect, that no oxigen is present in either. The following will

will serve farther to establish the former fact, and will place the latter in a stronger light.

These sulphurets cannot be formed by arsenious acid & sulphur, till the oxigen is given out.

It is very certain, that, if arsenic were in the state of oxide in these compounds, they might easily be formed by employing arsenious acid and sulphur. But on heating these substances together in a retort, &c., we obtain for a long time nothing but sulphurous acid: it is not till this gas nearly ceases to come over, that orpiment or realgar is formed. It may be said indeed, that arsenic is less oxided in these sulphurets, than in arsenious acid. But the existence of such oxides has never been proved. When arsenious acid is reduced by any method whatever, even by means of hidrogen gas, nothing is ever obtained but arsenious acid and arsenic, suspend the process at what period of it you please: and probably, if there were any fixed intermediate degrees of oxidation, they would be detected by proceeding in this way. Be this as it may, by combining sulphur with arsenic in different proportions in close vessels, we obtain at pleasure orpiment or realgar.

No intermediate state of oxidation.

Distinguishing properties of the two sulphurets.

Three parts of sulphur and four of arsenic form orpiment: one of sulphur and three of arsenic form realgar. Realgar enters into fusion at a very low temperature, and continues fluid long after the retort is withdrawn from the fire. Orpiment requires a somewhat higher heat to fuse it. Both rise by sublimation, and adhere to the neck of the retort. The orpiment is transparent, and of a hyacinth colour, so that at first it might be taken for a sort of realgar: but native orpiment itself assumes this colour on being melted; and both, that is the native orpiment after its beautiful colour has been thus changed, and the artificial, become of a very pure and lively yellow by pulverization. It is not the same with the orpiment produced in the humid way. The colour of this is similar to that of native orpiment that has never been exposed to heat: and it is in every respect similar to it, whether it be the product of a mixture of arsenious acid and sulphuretted hidrogen, or of a soluble arsenite, hidrosulphuret, and an acid.

Thus it appears demonstrated, that yellow orpiment in shining scales, and even endued with a sort of elasticity, is formed in some fluids; while realgar is produced by arsenic and

Orpiment sometimes may be mistaken for realgar.

and sulphur melted together: and that, since orpiment assumes a hyacinth colour on fusion, similar compounds may possibly exist in nature, and have been mistaken for realgar.

Neither contains oxigen.

However this may be, it is established beyond a doubt, that both orpiment and realgar contain no oxigen: they are sulphurets of arsenic more or less sulphuretted. In orpiment the arsenic is to the sulphur in the proportion of four to three; in realgar, in that of three to one. If more than three parts of sulphur be combined with four of arsenic, a yellow compound is obtained, the colour of which is not very lively, and approaches more or less to that of sulphur: in like manner, if less than one part of sulphur be united with three of arsenic, a compound of a browner colour than common realgar is formed: and as sulphur and arsenic are capable of combining together in a great number of different proportions, the shades that sulphuret of arsenic may present to us must be very numerous.

Orpiment 4 parts arsenic and 3 sulphur. Realgar 3 arsenic and 1 sulphur. But sulphur & arsenic combine in various intermediate proportions.

SCIENTIFIC NEWS.

Decomposition of the Alkalies.

The alkalis decomposed.

THE suggestions of Mr. Davy, in his observations on the agencies of electricity, which we have already given in this number, see p. 62, have been in some measure verified by that ingenious and learned gentleman, and produced very surprising results. Moistened potash and soda, exposed on a plate of platina to the action of the galvanic circle, have been decomposed into oxigen and a base, that in some of its properties resembles metals. Thus we find oxigen has no more claim to be considered as the generator of acids, than as that of alkalis, for it appears to make a part of ammonia likewise. The base too is highly inflammable, and forms an amalgam with mercury: but it is so far from having the specific gravity of metals, that it is ligher than most fluids. The base of potash has a specific gravity of 0 6 only: at the freezing

Into oxigen and an inflammable base.

Properties of the base of potash.

freezing point it is hard, brittle, and when broken exhibits facets, as if crystallized, when examined by the microscope: at 40° it is scarcely distinguishable from a small globule of quicksilver, at 60° is quite fluid, and at 100° evaporates. It is extremely greedy of oxigen, absorbing it rapidly from the atmosphere, and resuming the alkaline state. Yet if amalgamated with twice its bulk of quicksilver, and applied in the circuit of a powerful battery to iron, silver, gold, or platina, these metals are immediately dissolved, and converted into oxides, while the alkali is regenerated. Glass is dissolved by it in the same manner as the metals. A globule placed on a piece of ice burnt with a bright flame and intense heat, and potash was found in the water from the melted ice. In this case, as well as when a globule was thrown into water, a considerable quantity of hidrogen was rapidly evolved. When a globule was placed on a piece of moist turmeric paper, it appeared instantly to acquire intense heat, but moved so rapidly in quest of the moisture, that the paper was no where burned; but a deep red stain, that marked its course, proved the regeneration of the alkali.

Base of soda.

The base of soda is somewhat heavier than that of potash, its specific gravity being 0·7. It remains solid in a temperature not exceeding 150°, but at 180° it is perfectly fluid.

Proportions of oxigen in the alkalis.

From a considerable number of experiments potash appears to consist of 85 parts base and 15 oxigen, and soda of 80 parts base and 20 oxigen. It would seem too, that ammonia contains 20 per cent of oxigen; but this proportion was deducted from more complicated calculations, and less direct experiments.

Strontia and barytes.

On examining strontia and barytes, oxigen was educed from both of them.

Economical and medicinal uses of chinese radish oil.

Chinese radish introduced into Italy.

ABOUT fifteen years ago Mr. de Grandi, member of the Patriotic Society of Milan, introduced and established the cultivation

cultivation of a species of radish, the *raphanus sinensis*. The culture of this plant has been attended with such success, as to merit attention.

Yields an excellent oil for the table, or lamps.

The Chinese radish yields a large quantity of oil; and experiments lately made at Venice show, that this oil is preferable to any other kind known, not only for culinary purposes, and giving light, but in medicine.

Useful in medicine, and keeps extraordinarily well.

From the experiments made by Dr. Francis di Oliviero, it is extremely useful in rheumatic and pulmonary affections; it is not liable to spoil by keeping like other oils; and it has been employed with much success in convulsive coughs.

Culture.

The plant is not injured by the strongest frosts; it is sown in September, and in May or June the seed is gathered, which is very abundant.

Simple process for salting and smoking meat.

Process for making excellent hung beef in 48 hours.

In Franconia a method of salting and smoking meat is employed, that requires only eight and forty hours. The following is the process. A quantity of saltpetre, equal to the common salt that would be required for the meat in the usual way, is dissolved in water. Into this the meat to be smoked is put, and kept over a slow fire for a few hours, till all the water is evaporated. It is then hung up in a thick smoke for four and twenty hours, when it will be found equal in flavour to the best Hamburg smoked meat, that has been kept several weeks in salt, as red interiorly, and as firm.

To Correspondents.

I am sorry to inform my correspondent at Whitby, that his letter was unfortunately lost by the carelessness of the messenger employed to convey it to me from the publisher. If he has retained a copy of it therefore, I would request the favour of him, to transmit it to me.

Dr. Traill's letter will appear in our next number.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

FEBRUARY, 1808.

ARTICLE I.

On Albinoes: by T. S. TRAILL, *M. D.*

To Mr. NICHOLSON.

THE following account of a poor family in this town is transmitted for insertion in your Journal, if deemed singular enough to entitle it to a place in that valuable miscellany. The history was noted down a few days ago in my house from the words of the mother, who brought with her two of her children, who in all respects resembles the *Albinoes* of Chamouni, so well described by de Saussure in his *Voyage Dans les Alpes*. Albinoes.

Robert Edmond and his wife Anne are both natives of Anglesey in North Wales. He has blue eyes and hair almost black; her eyes are blue, and her hair of a light brown. Neither of them have remarkably fair skins. They have been married fourteen years. Their first child, a girl, had blue eyes and brown hair. The second, a boy, (now before me) has the characteristics of an *albino*: viz. very fair skin, flaxen hair, and rose-coloured eyes. The third and fourth children were twins, and both boys; one of them has blue eyes and dark brown hair; the other was an *albino*. The former is still alive: the albino lived nine months, though a very Their parents.

puny child. The fifth child, a girl, had blue eyes and brown hair. The sixth, and last now here, is a perfect albino.

The oldest described.

The oldest of these albinoes is now nine years of age, of a delicate constitution, slender, but well formed both in person and in features; his appetite has always been bad; he frequently complains of a dull pain in his forehead; his skin is exceedingly fair; his hair flaxen and soft; his cheeks have very little of the rose in them. The iris and pupil of his eyes are of a bright rose red colour, reflecting in some situations an opaline tinge. He cannot endure the strong light of the sun. When desired to look up, his eyelids are in constant motion, and he is incapable of fixing the eye steadily on any object, as is observed in those labouring under some kinds of slight ophthalmia, but in him it is unaccompanied by tears. His mother says, that his tears never flow in the coldest weather, but when vexed they are shed abundantly. The white of the eye is generally bloodshot. He says he sees better by candle than by daylight; especially at present, when the reflection from the snow on the ground is extremely offensive to him. He goes to school, but generally retires to the darkest part of it to read his lesson, because this is most agreeable to his eyes. In my room, which has a northern aspect, he can only distinguish some of the letters in the pages of the Edinburgh Review; but, if the light is not permitted to fall full on the book, he is able to read most of them. He holds the book very near his eye. His disposition is very gentle; he is not deficient in intellect. His whole appearance is so remarkable, that some years ago a person attempted to steal him, and would have succeeded in dragging him away, had not his cries brought a person to his assistance.

The younger.

The youngest child is now nine months old; is a very stout, lively, noisy, and healthy boy. In other respects he perfectly resembles his brother.

Approach to it in a relation.

The mother says, that one of her *cousins* has a very fair skin, flaxen hair, and very weak light blue eyes.

Supposed want of the black mucus in the eye.

Professor Blumenbach of Gottingen, in a curious memoir read before the Royal Society of that city, endeavoured to prove, that the red colour of the eyes of the albinoes of Chamouni was owing to the want of *pigmentum nigrum* within the

the eye. About the same time, Buzzi of Milan had an opportunity of dissecting an albino, and proved, that the *pigmentum nigrum* of the choroid coat, and also that portion of it which lies behind the iris, and is called uvea by anatomists, were wanting; thus demonstrating what Blumenbach had supposed. This deficiency was observed before by Blumenbach in some white dogs, owls, and in white rabbits. Buzzi discovered, that the layer of the skin called *rete mucosum* was also wanting, and to this he with great probability attributes the peculiar fairness of the skin; the colouring matter of the negro, and of the hair of animals, being lodged in this membrane.

Proved by dissection.

Wanting in some white animals too.

Rete mucosum absent.

It is well known, that from the tawny natives of Asia, Africa, and America, albinoes sometimes spring, who are said to be capable of propagating a race like themselves, when they intermarry. Whether this be the case with the albinoes of Europe is unknown; for, as far as I have been able to learn, not one of them was a female. There are on record eight instances of European albinoes, beside the three now noticed. Two of these are described by Saussure, four by Buzzi, one by Helvetius, and one by Maupertuis, all of whom were males. The parents of the two young men of Chamouni had female children of the usual appearance. The woman of Milan had seven sons, three of whom were albinoes. Mrs. Edmond's girls were all of the usual appearance, but all her boys were albinoes. Among these eleven cases not one albino girl has been found. This at least proves, that males are more subject than females to this singular structure.

Albinoes from tawny parents continue their race.

European albinoes generally males.

From the perpetuation of this variety of the human species in Java, Guinea, and other places, as well as from the account Mrs. Edmond gives of her cousin, it would seem to be hereditary.

This variety becomes hereditary.

The causes which produce it are like those which produce defects of limbs, or of various viscera, wholly concealed from our curiosity. Buzzi relates, that the woman of Milan, when pregnant with the albinoes, always had an immoderate longing for milk, which she used to excess; but never felt that desire while pregnant with her other children; and he seems to ascribe this longing to some *internal heat or disease*.

Its cause unknown.

 Mrs.

Mrs. Edmond neither experienced any sensation, which could lead her to distinguish between each kind of foetus; nor was her general health sensibly affected in one case more than in the other. The story of the milk, so much resembles those invented by our own good ladies to explain *nævi materni*, or those singular marks which are sometimes observable on the bodies of children, that I am not disposed to pay much attention to it. With regard to the supposed internal disease, which Buzzi imagines destroyed the *rete mucosum* of the albino foetus, it is difficult to conceive any disease of the mother capable of producing so extensive an effect on one of Mrs. Edmond's children, while its twin brother was altogether free from any mark of the existence of such malady. Beside this, the regular alternation of the albinoes with her other children does not favour the notion of their peculiarities arising from disease on the system of the mother. De Saussure very properly rejects the idea of this conformation being produced by the air of mountainous regions. The three albinoes I have just described were born near the sea, on the extensive plains of Lancashire, and the birthplace of the parents is the flat island of Anglesey. Where facts are so few, and the causes seemingly so remote from human investigation, it is better to rest satisfied with having observed them, than to waste time on useless hypothesis.

Not connected with a mountainous region.

THOMAS STEWART TRAILL.

Liverpool, Dec. 9, 1807.

ANNOTATION.

Instance of an English albiness.

Dr. Traill justly remarks the singularity, that of all the cases of European albinoes on record not one should be a male. Most of my London readers, however, will be aware, that a female of this description has been exhibited in the metropolis for some years, and is at present at the rooms in Spring Gardens. She answers exactly to the full and accurate description of the boy given above. Her hair, I think, which she suffers to grow very long, has more of a silky appearance than that of the two male albinoes exhibited here

here about twenty years ago, at least to the best of my recollection, and more of the yellow tinge of raw silk. She does not see better in the dark than other people, but on the contrary not so well as most. She is a native of Essex, and I apprehend between twenty and thirty years old; perfectly well shaped, about the middle size, and says she has always been very healthy, which her appearance does not any way contradict. In her understanding she seems by no means deficient.

She informs me, that her mother's first child, a girl, is also an albiness like herself; that she was the third child; and that the fifth, a boy, is an albino. The two intermediate children had nothing remarkable. Her mother had never any peculiar longing, ailment, or fright, she added, during either of her pregnancies. A second instance.

Another instance of a female in my own knowledge is the eldest daughter of a respectable tradesman in London, about three and twenty, who has a brother an albino, about ten years younger than herself. She was the first child of her parents, the boy the last, and none of the intermediate children had any thing peculiar in their appearance. A third.

I am farther informed, that two albinesses, both young, are now exhibiting about the country with their brother, who is an albino. They are said to be natives of Ireland, but I have not been able to get any certain information respecting them. Two more.

I likewise remember an albiness, perhaps eight or nine years old, being introduced one evening to the society at Guy's Hospital about twenty years ago; and had supposed it might be the same person as is now to be seen at Spring Gardens: but she assures me, that neither she nor her sister had ever been shown at Guy's, or any other place, till she began to be exhibited in public a few years ago. Thus there would appear to have been no less than six females of this description born in the United Kingdom within these thirty years: and *if* none have been noticed by writers, it is probably to be ascribed to the greater care, with which women endeavour to conceal any thing they would consider as a personal blemish, or to shun the view of strangers, when A sixth.

marked

marked by any thing singular. In confirmation of this it may be added, that the young lady I have mentioned conceals her peculiarity as much as possible by wearing a wig that falls down over her eyebrows, and a bonnet as large as fashion will allow.

II.

Description of a new Eudiometer, accompanied with Experiments, elucidating its Application. By WILLIAM HASLEDINE PEPYS, *Esq. Communicated by* CHARLES HATCHETT, *Esq. F.R.S**.

Atmospheric air of great importance in various natural and artificial processes.

THE important part which atmospheric air performs, in maintaining the principle of life in animals, in combustion of every description, the acidification, and oxidation of a great variety of substances, and in numerous other processes both of nature and art, gives a high degree of interest to every thing calculated to extend our knowledge of its nature and properties.

Many other aeriform fluids.

The evidence furnished by modern chemistry, of the existence of many other aeriform substances, increases this interest, especially when it is considered that, owing to their possessing some of the most obvious properties of atmospheric air, as transparency, elasticity, and a power of great expansion, on being exposed to an increase of temperature, they were with very few exceptions, till lately, confounded either with common air, or not even suspected to exist.

Frequently evolved when little expected.

When to these considerations we add the facility, with which some products, especially the gaseous, are evolved, in circumstances under which, in the present state of our knowledge, we should hardly look for them; the power they possess of decomposing each other, and, by an interchange and new arrangement of principles, of producing compounds, possessing properties altogether different from those of the ingredients supposed to be present; and the facilities which every new detection of unsuspected principles

* From the Philosophical Trans. for 1807, Part II, p. 247.

affords,

affords, toward the discovery of others, and consequently the composition, or analysis of bodies before held to be simple, it will not appear a matter of surprise, that the subject of eudiometry should have obtained a considerable degree of attention from modern philosophers.

Hence eudiometry claims much attention.

This would be an improper place to enumerate all that has been done, or proposed, by different men of eminence, towards the production of something like a perfect system on this important subject; yet some allusion to their labours appears to be indispensible, and will be the means of preventing some circumlocution in our farther progress.

What has been done on the subject.

Hales* appears to be the first who observed absorption to take place in common air, on mixing it with air obtained from a mixture of *Walton pyrites* and *spirit* of *nitre*; and that in this process from being clear they became "a reddish turbid fume."

Hales.

Dr. Priestley, as he informs us in his *Observations on different kinds of Air*†, was much struck with this experiment, but never expected to have the satisfaction of seeing this remarkable appearance, supposing it to be peculiar to the Walton pyrites, till encouraged by a suggestion of Mr. Cavendish, that probably the red appearance of the mixture depended upon the spirit of nitre only, he tried solutions of the different metals in that acid, and, catching the air which was generated, obtained what he wished. To the air thus produced he gave the name of *nitrous* air; and, from its possessing the properties of absorbing that portion of atmospheric air which he calls *dephlogisticated*, first proposed its being used as a test for ascertaining the purity of air. His method of proceeding was ingenious and simple; known quantities of the air to be tried, and of nitrous gas, being mixed, were admitted, after the diminution of volume occasioned by their union, into a graduated tube, which he denominated a *eudiometer*.

Priestley.

The first eudiometer.

It was with the test of nitrous gas, that Mr. Cavendish‡ made his masterly analysis of the air at Kensington and London; and by many laborious processes and comparative

Cavendish's analysis of common air.

* Statical Essays, Vol. I, p. 224; Vol. II, p. 280.

‡ Phil. Trans. for 1783. † Phil. Trans. for 1772, p. 210.

trials

trials obtained results, the accuracy of which has been more distinctly perceived, the more the science of chemistry has advanced.

Phosphorus and sulphuret of potash employed.

The slow combustion of phosphorus, which unites with the oxigen to form an acid, and the decomposition of the fluid sulphuret of potash, are certain methods of separating combinations consisting of oxigen and azote: but the decomposition is effected so slowly, by the action of these substances, that it became a desirable object, to discover some means for accelerating the process. This was supposed to have been effected by Guyton, who proposed heating the sulphuret of potash; in doing this, sulphuretted hidrogen gas however is frequently evolved, which, mixing with the residual gas, increases its quantity, and renders the result fallacious.

Guyton.

Davy.

Sulphate of iron impregnated with nitrous gas.

The green sulphate of iron impregnated with nitrous gas, first discovered by Dr. Priestley, and recently used by Mr. Davy for eudiometrical purposes, from its possessing the property of absorbing oxigen gas from the atmosphere, is much to be preferred to the method with nitrous gas, as the green sulphate of iron does not combine with the other gasses, with which the nitrous gas is commonly found to be contaminated, and more certain results are obtained.

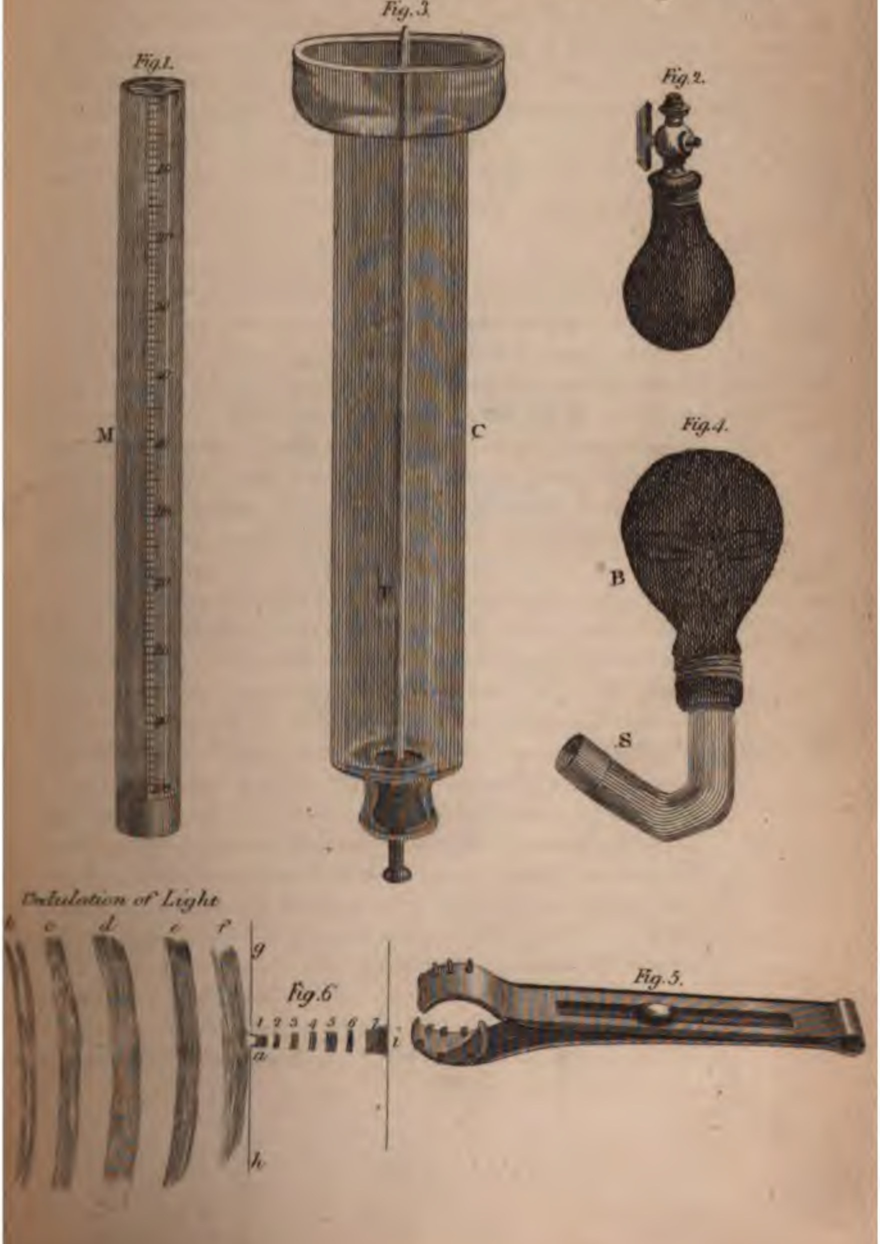
A correct and commodious apparatus still wanting.

Having had occasion to repeat many of the experiments of others, and to make some new ones, I soon found what every one, who has been engaged on the same subject, must have experienced; that an apparatus more commodious than has yet been proposed, and at the same time capable of giving correct results, with the greatest minuteness, was still a desideratum in eudiometry. To detail the various ideas, that presented themselves on the subject, would be an unnecessary encroachment on the time of this Society: but as I at last succeeded in contriving an instrument, possessing the above properties in a very eminent degree, I flatter myself I shall not be thought intrusive, in offering a description of it.

Description of that invented by the author.

This apparatus, which is of easy construction, and extremely portable, consists of a glass measure M, pl. III, fig. 1, graduated into hundred parts; a small gum elastic bottle B, fig. 4, capable of containing about twice the quantity of the measure, and furnished with a perforated glass stopper, S,

New Eudiometer by W. H. Pepys Esqr.



S, which is well secured in the neck of it, by means of waxed thread wound tight round it: and a glass tube, T, fig. 3, also graduated, but into tenths of the former divisions, or into thousandth parts of the measure.

The glass stopper, made fast in the neck of the gum elastic bottle, as above mentioned, has its exterior end ground with emery, exactly to fit the mouth of the measure. To the lower end of the graduated tube T is cemented a small steel cock, which is secured into the neck of a very small gum elastic bottle, fig. 2, by means of waxed thread. The other end of the tube is conical, so as to present a very small orifice.

Beside this, the apparatus is furnished with a kind of movable cistern C, in which the tube can be slid easily up and down, and yet in such a manner, that the water or other liquid in the cistern may not pass. This is easily accomplished by means of a cork fitted into its mouth, with a perforation through its axis to receive the tube. The cistern, when in use, is to be filled with water, or mercury, as the experiment may require, and becomes a secondary cistern for the measure, as will be more clearly understood, by the following description of the method of performing experiments with this instrument.

The measure is filled with the air, or gas, over mercury in the usual manner; and the elastic bottle is charged with the solution, intended to be employed as the reagent: the orifice of the stopper is then inserted into the mouth of the measure, in the mercury, and pressed home to its place. Manner of using it.

The bottle and measure, being thus united, are to be firmly held at the joint. Upon pressing the former, a portion of the fluid is injected into the latter, and the gas suffers a degree of compression, by which the action of the affinity, between it and the fluid, is accelerated. On taking off the pressure, the bottle, by its elasticity, endeavours to obtain its original form, and receives back the fluid. This process should be continued as long as any absorption is observed to take place. When absorption ceases, the bottle is to be separated from the measure under mercury, and the quicksilver which remains in the measure being brought to the

the level of that in the cistern, the quantity of absorption is then to be determined, which is done as follows:

Method of ascertaining the absorption.

Suppose atmospheric air has been the subject of the experiment, and consequently a large residuum left: first note the hundredth parts: and then, to obtain a knowledge of the fractional parts, remove the measure into the small cistern, in which the graduated tube filled with mercury is placed; slide the tube above the surface of the fluid in the measure, and, opening the stop-cock, suffer the mercury to descend, till it has drawn the fluid in the measure to a regular division; then stop the cock, and register the hundredth parts on the measure, and the thousandth parts on the graduated tube; the united quantities give the sum of the residual gas. Observe well in registering the thousandth parts, that the fluids are exactly on a level, on the outside and the inside of the measure; this may be easily effected, by pouring out a portion of the liquid of the small cistern, or adding thereto.

When the residuum is very small.

If instead of atmospheric air, a gas is tried, which so far as it is uncontaminated can be nearly wholly absorbed by the reagents employed, the process becomes exceedingly simple; for if the residuum is under a hundredth part of the measure, it may be transferred completely into the graduated tube, and its quantity at once ascertained.

Advantage of the bent tube.

The stopper S would have injected the fluid with greater velocity had it been straight; but it would not then have been so convenient in the analysis of compound gasses, where both mercury and hot solutions are occasionally employed; as the mercury would have so compressed the fluid in the bottle, in introducing it under that metal, as to have thrown out a portion of its contents, and also have robbed the hot solutions of the temperature, which was necessary to their perfect action.

Mode of reducing the measure to a proper size,

As to the size of the measure M, I have generally preferred the cubic inch divided into hundredth parts. This is easily effected by taking a stout glass tube about half an inch calibre, sealing one end, then weighing 3422 grains of mercury, equal to 252 grains of distilled water at temperature 50° Fahrenheit. This is introduced into the tube; the extra length is cut off with a sharp-edged file, care being taken to leave a sufficient portion to grind the perforated stopper S into its mouth.

The

and graduating it.

The divisions are obtained by a small measure, made from a glass tube sealed at the end, and cut off exactly to the hundredth part of a cubic inch, equal to 34·2 grains of mercury, which, being ground flat, is stopped by a piece of plate-glass, and the divisions marked by the diamond, upon the introduction of each hundredth part of mercury into the measure M.

Mode of graduating the tube.

The tube T is divided into tenths of the measure M, or thousandth parts of a cubic inch. This is done by measuring one hundredth part of a cubic inch into the tube, and dividing it into ten parts, marking the divisions with fluoric acid, or black enamel.

To prove the accuracy of the instrument, I shall proceed to relate a few experiments made with it.

Experiments to prove its accuracy.

The elastic bottle being filled with the solution of sulphate of iron impregnated with nitrous gas, and the measure with atmospheric air, they were united, and by gentle injection $\frac{214}{1000}$ were absorbed.

If the experiment is made hastily, the impregnated solution loses a portion of its nitrous gas, which must be again absorbed by a solution of green sulphate of iron.

For ascertaining the purity of nitrous gas, the bottle may be charged with the solution of green sulphate or muriate of iron.

For carbonic acid gas, with lime or barytic water.

For oxigen gas*, with the solution of green sulphate of iron impregnated with nitrous gas.

For sulphuretted hidrogen gas, a solution of nitrate of silver was put into the elastic bottle, and sulphuretted hidrogen gas† into the graduated measure. Upon the first injection, the solution took a black flocculent appearance, and a considerable portion of the gas was absorbed. After repeating the process as before mentioned, the residuum was $\frac{1}{1000}$.

Mixed gasses may be analysed by it.

The instrument may be likewise generally applied to the analysis of mixed gasses.

I have been able completely to separate the carbonic acid gas from the sulphuretted hidrogen, by a solution of the ni-

* Obtained from oximuriate of potash by heat.

† Obtained from sulphuret of potash by diluted muriatic acid, and collected and preserved with the greatest care.

trate

trate of silver, or of mercury employed hot. The carbonic acid gas is expanded in this process, but on standing over mercury it returns to its original volume. The sulphuretted hidrogen, in this instance, is taken up by the metallic nitrate.

Carbonic acid gas decomposes acetite of lead. It should be here observed, that the acetite of lead must not be used, as the carbonic acid gas, even at a high temperature, decomposes it, forming carbonate of lead.

Why the solutions should be used hot. The propriety of using the solutions hot will be seen, when we recollect, that the carbonic acid gas is soluble in the water of solution at the common temperature of all these compounds.

Nitrous gas and carbonic acid gas. Nitrous gas, and carbonic acid gas, may be separated by means of the hot solution of the green sulphate of iron. To effect this, heat a solution on a glass capsule over a spirit lamp until ebullition. Having filled the measure with the compound gas, charge the elastic bottle with the hot solution, and unite them. The nitrous gas in two or thee injections will be absorbed, changing the colour of the solution, while the carbonic acid gas will be a little rarefied, but no absorption of it will take place.

Gasses absorbed by alcohol, Previous to these experiments on the compound gasses, I had tried several on the carbonic acid, sulphuretted hidrogen, and nitrous gasses in their unmixed states. One hundred parts of pure alcohol at the common temperature will absorb 70 parts in volume of carbonic acid, and the same quantity of sulphuretted hidrogen. Alcohol impregnated with the latter precipitates the solutions of the nitrates of lead, silver, and mercury, of a dark brown colour. Nitric acid of the specific gravity 1·4, and also of 1·25, absorbs carbonic acid gas, without any apparent change in the nitric acid. Sulphuretted hidrogen gas is also absorbed by nitric acid, which occasions a slight milky cloud or precipitate therein.

by nitric acid,

and by nitrates. The solutions of nitrates of barytes, strontian, and lime, absorb carbonic acid gas equal to half their volume, without any apparent alteration.

Solutions of nitrates of barytes, strontian, and lime, also absorb sulphuretted hidrogen gas, equal to $\frac{6}{10}$ of their volume, with a slight change of colour; the solutions thus impregnated precipitate solutions of nitrate of mercury and of

of silver, and acetite of lead, of a dark brown colour, and would be useful as chemical reagents.

Carbonic acid gas, as I have before stated, decomposes solutions of the acetite of lead, hot or cold, forming a precipitate of carbonate of lead.

Carbonic acid gas is absorbed by the solution of the green sulphate of iron, under the temperature of 100° Fahrenheit: but this is only the action of the water of solution. If the temperature be near boiling, or above 180° Fahrenheit, the solution increases the volume of the gas without the slightest absorption; after carbonic acid gas has in this way been treated with the hot solutions, it is still soluble in water at the common temperature, or in aqueous solutions of lime, or alkali.

Carbonic acid gas absorbed only by the water in solution of sulphate of iron.

Nitrous gas is absorbed by solution of sulphuret of potash, with a separation or formation of sulphur. Upon injecting the solution the sides of the measure take a milky appearance, which on the second injection is washed down, insoluble in the liquor. About 80 parts from 100 of gas are absorbed.

Nitrous gas.

Nitrous gas is also absorbed by nitrate of copper in solution, without any peculiar alteration.

In these experiments, great care must be taken not to increase the temperature of the gas by the hand. To prevent this I use a pair of small circular-mouthed forceps, lined with cloth, which firmly grasp the measure, fig. 5; and if the experiments should in any way be delayed, a corresponding manometer will always be sufficient to correct the errour occasioned by change of atmospheric temperature and pressure.

Forceps used to prevent increase of temperature from the hand.

To ascertain the quantity of carbonic acid gas, contained in oxigen gas (of a known purity,) after combustion, or decomposition of carbonaceous substances, lime water will be found sufficient.

Examination of oxigen for carbonic acid after combustion,

If it is required to know the purity of the oxigen gas, after the carbonic acid gas has been absorbed, the best method, and the least liable to errour, is to withdraw the residual oxigen gas, by means of the small graduated tube before described.

and for other gasses.

To do this, remove the measure into the small cistern of mercury;

mercury; press the quicksilver out of the small bottle by the fingers and thumb, and let the tube rise a sufficient height within the measure, that the bottle extending itself shall withdraw the whole of the gas from the measure, taking care that the cock be stopped as soon as it has completed it, and also to prevent the solution from entering the tube.

If the opening of the tube is small, it may then be drawn down into the mercury, without the possibility of any portion of the gas escaping, while the measure is dried or cleaned, or a fresh one filled with mercury supplied to receive it.

Convenience of this mode of transferring gasses.

This way of transferring will be found very advantageous, particularly in the separation of gasses liable to be absorbed under certain temperatures; and also where a new series of reagents is to be employed, as from the depositions of former solutions on the glass measure a source of considerable errour would arise.

Farther instructions for using the apparatus.

The residual oxigen gas being thus transferred into a clean dry measure, the processes before described for examining oxigen gas may be then used; or the quantity of carbonic acid gas (for examination) being found by lime water, another measure of the gas may be tried, first with the green sulphate of iron impregnated with nitrous gas, and then with the green sulphate in solution only: these will take up both the carbonic acid gas, and the oxigen gas, leaving only such residual gas as the oxigen might have originally contained.

Transferring is not here necessary, as the two solutions may be used one after the other, taking care to use the solution of green sulphate last.

Where it is not requisite to transfer the gas into a dried or clean measure, previous to the use of another solution, as in the instance I have just mentioned, a quantity of the first solution may be withdrawn, by simply filling the elastic bottle with mercury, then joining it to the measure, and by inclining the measure, the mercury by its gravity will displace the former solution.

If at any time the gas should get drawn into the elastic bottle, it may be very easily returned into the measure, by inclining sometimes the bottle, and sometimes the measure. The only errour that could arise from this is, an increase of temperature in the gas, which may be rectified, by plunging the

the whole apparatus into mercury, or water, of the standard temperature.

The advantages of this construction of the eudiometer will be readily perceived by all those, who are in the habit of making chemical experiments. The portion of gas to be examined is completely under command; it may be agitated without the least fear of the intrusion of any atmospheric air, and the process thereby very materially shortened. The gum elastic is a substance so little acted upon by chemical agents, that a great variety may be employed; and above all, we can very conveniently use hot solutions, which will be found an important auxiliary in the examination of some compound gasses.

Advantages of this eudiometer.

Simple as this instrument may appear, it is calculated to extend our knowledge of the different kinds of air, by the precision and accuracy which it enables us to obtain, and which solely constitute the value of every experiment. A degree of confidence is inspired from knowing, that we can depend upon our results; and hence much valuable time, which would have been wasted in uncertain, if not useless investigations, may be directly applied to the advancement of science.

III.

On the Revival of an Obsolete Mode of managing Strawberries. By the Right Hon. Sir JOSEPH BANKS, *Bart. K. B. P. R. S. &c.**

THE custom of laying straw under strawberry plants, when their fruit begins to swell, is probably very old in this country: the name of the fruit bears testimony in favour of this conjecture, for the plant has no relation to straw in any other way, and no other European language applies the idea of straw in any shape to the name of the berry, or to the plant that bears it.

Straw formerly laid under strawberry plants in this country.

Hence the name.

When Sir Joseph Banks came to Spring Grove, in 1779, he

Practised with.

* From the Transactions of the Horticultural Society, Vol. I, Part I, p. 54.

In these 30 years.

he found this practice in the garden: John Smith, the gardener, well known among his brethren as a man of more than ordinary abilities in the profession, had used it there many years; he learned it soon after he came to London from Scotland; probably at the Neat Houses, where he first worked among the market gardeners, it is therefore clearly an old practice, though now almost obsolete.

Attended with various advantages.

Its use in preserving a crop is very extensive: it shades the roots from the sun; prevents the waste of moisture by evaporation, and consequently, in dry times, when watering is necessary, makes a less quantity of water suffice than would be used if the sun could act immediately on the surface of the mould; besides, it keeps the leaning fruit from resting on the earth, and gives the whole an air of neatness as well as an effect of real cleanliness, which should never be wanting in a gentleman's garden.

Expense of the practice

The strawberry beds in that garden at Spring Grove, which has been measured for the purpose of ascertaining the expense incurred by this method of management, are about 75 feet long, and five feet wide, each containing three rows of plants, and of course requiring four rows of straw to be laid under them. The whole consists of 600 feet of beds, or 1800 feet of strawberry plants, of different sorts, in rows. The strawing of these beds consumed this year, 1806, the long straw of 26 trusses, for the short straw being as good for litter as the long straw, but less applicable to this use, is taken out; if we allow then, on the original 26 trusses, six for the short straw taken out and applied to other uses, 20 trusses will remain, which cost this year 10*d.* a truss, or 16*s.* 8*d.* being one penny for every nine feet of strawberries in rows.

a mere trifle.

The straw makes manure:

From this original expenditure the value of the manure made by the straw when taken from the beds must be deducted, as the whole of it goes undiminished to the dunghill as soon as the crop is over. The cost of this practice therefore cannot be considered as heavy; in the present year not a single shower fell at Spring Grove, from the time the straw was laid down till the crop of scarlets was nearly finished, at the end of June. The expense of strawing was therefore many times repaid by the saving made in the labour

and much labour and water

bour of watering, and the profit of this saving was immediately brought to account in increase of other crops, by the use of water spared from the strawberries; and besides, the berries themselves were, under this management, as fair and nearly as large as in ordinary years, but the general complaint of the gardeners this year was, that the scarlets did not reach half their natural size, and of course required twice as many to fill a pottle as would do it in a good year. saved by it in dry seasons.

In wet years the straw is of less importance in this point of view, but in years moderately wet, the use of strawing sometimes makes watering wholly unnecessary, when gardeners who do not straw are under the necessity of resorting to it; and we all know if watering is once begun, it cannot be left off till rain enough has fallen to give the ground a thorough soaking. In moderately wet years renders watering unnecessary.

Even in wet years the straw does considerable service, heavy rains never fail to dash up abundance of mould, and fix it upon the berries, this is entirely prevented, as well as the dirtiness of those berries that lean down upon the earth, so that the whole crop is kept pure and clean: no earthy taste will be observed in eating the fruit that has been strawed, and the cream which is sometimes soiled when mixed with strawberries, by the dirt that adheres to them, especially in the early part of the season, will retain to the last drop that unsullied red and white, which give almost as much satisfaction to the eye while we are eating it, as the taste of that most excellent mixture does to the palate. And in wet years is of considerable service.

IV.

On raising new and early Varieties of the Potato (Solanum Tuberosum). *By* THOMAS ANDREW KNIGHT, *Esq. F.R.S. &c**.

THE potato contributes to afford food to so large a portion of the inhabitants of this country, that every improvement in its culture becomes an object of national impor-

* From the Trans. of the Horticultural Society, vol 1, p. 1, p. 57.

tance; and thence I am induced to hope, that the following communication may not be unacceptable to the Horticultural Society.

Early potatoes without blossoms.

Every Person, who has cultivated early varieties of this plant, must have observed, that they never afford seeds, nor even blossoms; and that the only method of propagating them is by dividing their tuberous roots: and experience has sufficiently proved, that every variety, when it has been long propagated, loses gradually some of those good qualities, which it possessed in the earlier stages of its existence. Dr. Hunter, in his Georgical Essays, I think has limited the duration of a variety, in a state of perfection, to about fourteen years: and probably, taking varieties in the aggregate, and as the plant is generally cultivated, he is nearly accurate. A good new variety of an early potato is therefore considered a valuable acquisition by the person, who has the good fortune to have raised it; and as an early variety, according to any mode of culture at present practised, can only be obtained by accident from seeds of late kinds, one is not very frequently produced: but by the method I have to communicate, seeds are readily obtained from the earliest and best varieties; and the seeds of these, in successive generations, may, not improbably, ultimately afford much earlier and better varieties, than have yet existed.

Degenerate from continued propagation by roots.

Varieties continue in perfection about 14 years.

Early potatoes fail to seed, from soon forming tubers

I suspected the cause of the constant failure of the early potato to produce seeds, to be the preternaturally early formation of the tuberous root; which draws off, for its support, that portion of the sap, which, in other plants of the same species, affords nutriment to the blossoms and seeds: and experiment soon satisfied me, that my conjectures were perfectly well founded.

I took several methods of placing the plants to grow, in such a situation, as enabled me readily to prevent the formation of tuberous roots; but the following appearing the best, it is unnecessary to trouble the Society with an account of any other.

Method of preventing this.

Having fixed strong stakes in the ground, I raised the mould in a heap round the bases of them; and in contact with the stakes, on their south sides, I planted the potatoes from which I wished to obtain seeds. When the young plants

plants were about four inches high, they were secured to the stakes with shreds and nails, and the mould was then washed away, by a strong current of water, from the bases of their stems; so that the fibrous roots only of the plants entered into the soil. The fibrous roots of this plant are perfectly distinct organs from the runners, which give existence, and subsequently convey nutriment, to the tuberous roots; and as the runners spring from the stems only of the plants, which are, in the mode of culture I have described, placed wholly out of the soil, the formation of tuberous roots is easily prevented; and whenever this is done, numerous blossoms will soon appear, and almost every blossom will afford fruit and seeds. It appears not improbable, that by introducing the farina of the small, and very early varieties into the blossoms of those of larger size, and somewhat later habits, moderately early varieties, adapted to field culture, and winter use, might be obtained; and the value of these to the farmer in the colder parts of the kingdom, whose crop of potatoes is succeeded by one of wheat, would be very great. I have not yet made any experiment of this kind; but I am prepared to do it in the present spring.

The proper root distinct from the runners.

Moderately early varieties perhaps obtainable by mixture.

V.

An Account of two Children born with Cataracts in their Eyes, to show that their Sight was obscured in very different Degrees; with Experiments to determine the proportional Knowledge of Objects acquired by them immediately after the Cataracts were removed. By EVERARD HOME, *Esq. F.R.S**.

MR. Cheselden's observations on this subject, recorded in the Phil. Trans. for the year 1728, pointed out two material facts; that vision alone gives no idea of the figure of objects, or their distance from the eye, since a very intelligent

Cheselden's observations.

* Phil. Trans. for 1806, Part I, p. 83.

boy,

boy, 13 years of age, upon recovering his sight was unable to distinguish the outline of any thing placed before him, and thought that every object touched his eye.

Ware's in opposition to them.

Mr. Ware's cases, which have also a place in the Phil. Trans. for 1801, and are compared with that of Mr. Cheselden, appear to lead to a different conclusion. The following observations are laid before the Society with a view to explain this circumstance.

Boy born blind.

CASE 1. William Stiff, twelve years of age, was admitted into St. George's Hospital under my care, on the 17th of July, 1806, with cataracts in his eyes, which, according to the account of his mother, existed at the time of birth. From earliest infancy he never stretched out his hand to catch at any thing, nor were his eyes directed to objects placed before him, but rolled about in a very unusual manner, although in other respects he was a lively child. The eyes were not examined till he was six months old, and at that time the cataracts were as distinct as when he was received into the hospital.

Distinguished light, & could discern the sun or a candle.

Previous to an operation being performed, the following circumstances were ascertained respecting his vision. He could distinguish light from darkness, and the light of the sun from that of a fire or candle: he said it was redder, and more pleasant to look at, but lightning made a still stronger impression on his eyes. All these different lights he called red. The sun appeared to him the size of his hat. The candle flame was larger than his finger, and smaller than his arm. When he looked at the sun he said it appeared to touch his eye. When a lighted candle was placed before him both his eyes were directed towards it, and moved together. When it was at any nearer distance than 12 inches, he said it touched his eyes. When moved further off he said it did not touch them; and at 22 inches it became invisible.

One of the cataracts extracted at 12 years of age.

On the 21st of July the operation of extracting the crystalline lens was performed on the left eye. The capsule of the lens was so very strong as to require some force to penetrate it. When wounded, the contents, which were fluid, rushed out with great violence. Light became very distressing to his eye, and gave him pain. After allowing the eyelids

lids to remain closed for a few minutes, and then opening them, the pupil appeared clear, but he could not bear exposure to light. On my asking him what he had seen, he said, "your head, which seemed to touch my eye:" but he could not tell its shape. He went to bed, and took an opiate draught: the pain in his eye lasted about an hour, after which he fell asleep. The whole of that day the light was distressing to his eye, so that he could not bear the least exposure to it. Effects of the operation.

On the 22d the eye-lids were opened to examine the eye. The light was less offensive. He said he saw my head, which touched his eye. There was so much inflammation on the eye-ball, that a leech was applied to the temple, and the common means for removing inflammation were used.

On the 23d the eye was less inflamed, and he could bear a weak light. The pupil was of an irregular figure, and the wounded cornea had not united with a smooth surface. He said he could see several gentlemen round him, but could not describe their figure. My face, while I was looking at his eye, he said was round and red.

On the 25th the inflammation had subsided, but on the 27th returned, and continued notwithstanding different means were employed for its removal, till the 1st of August, when it was almost entirely gone. On the 4th the eye was apparently so well, that an attempt was made in the presence of Mr. Cavendish and Dr. Wollaston to ascertain its powers of vision; but it was so weak, that it became necessary to shade the glare of light by hanging a white cloth before the window. The least exertion fatigued the eye, and the cicatrix on the cornea, to which the iris had become attached, drew it down so as considerably to diminish the pupil. From these circumstances nothing could be satisfactorily made out respecting the boy's vision. On the 11th a second attempt was made in the presence of Mr. Cavendish, but the pupil continued so contracted and irregular, and the eye so imperfect in its powers, that it became necessary a second time to postpone any experiments.

On the 16th of September the right eye was couched. This operation was preferred after what had happened to the other eye, in the hope that there would not be the same de- The other eye couched.

gree

gree of inflammation, and as the former cataract was fluid, there was every reason to believe that couching would in this instance be most efficacious.

Effects of this operation.

The operation gave pain, and the light was so distressing to his eye, that the lids were closed as soon as it was over, and he was put to bed. The consequent inflammation was not severe, but as soon as the fluid cataract, which had been diffused through the aqueous humour, was absorbed, the capsule of the lens was found to be opaque, and the sight consequently imperfect. The eyes were not examined with respect to their vision till the 13th of October, during which period the boy remained quiet in the hospital. On that day the upper part of the pupil of the left eye had in some measure recovered its natural state, and had become transparent, but the cicatrix in the cornea was more extensively opaque than before. The light now was not distressing to either eye, and when strong, he could readily discern a white, red, or yellow colour, particularly when bright and shining. The sun and other objects did not now seem to touch his eyes as before, they appeared to be at a short distance from him. The eye, which had been couched, had the most distinct vision of the two, but in both it was imperfect. The distance at which he saw best was five inches.

When the object was of a bright colour, and illuminated by a strong light, he could make out that it was flat and broad; and when one corner of a square substance was pointed out to him, he saw it, and could find out the other, which was at the end of the same side, but could not do this under less favourable circumstances. When the four corners of a white card were pointed out, and he had examined them, he seemed to know them, but when the opposite surface of the same card, which was yellow, was placed before him, he could not tell whether it had corners or not, so that he had not acquired any correct knowledge of them, since he could not apply it to the next coloured surface, whose form was exactly the same, with that, the outline of which the eye had just been taught to trace.

Another boy born blind.

Case II. John Salter, seven years of age, was admitted into St. George's Hospital on the 1st of October, 1806, under

der my care, with cataracts in both eyes, which according to the accounts of his relations had existed from his birth.

After he was received into the hospital, the following circumstances were ascertained respecting his vision. The pupils contracted considerably when a lighted candle was placed before him, and dilated as soon as it was withdrawn. He was capable of distinguishing colours with tolerable accuracy, particularly the more bright and vivid ones. Distinguished light & colours

On the 6th of October the left eye was couched. This operation was preferred to extraction, from a belief, that the cataracts were not solid, and as the injury done to the capsule by the operation would be less, there was not the same chance of inflammation, the disposition for which had been so strong in the former case. As the eye was not irritable, and was likely to be but little disturbed by this operation, every thing was previously got ready for ascertaining his knowledge of objects, as soon as the operation was over, should the circumstances prove favourable. The operation was attended with success, and gave very little pain. The eye was allowed ten minutes to recover itself: a round piece of card of a yellow colour, one inch in diameter, was then placed about six inches from it. He said immediately, that it was yellow, and on being asked its shape said, "Let me touch it, and I will tell you." Being told that he must not touch it, after looking for some time, he said it was round. A square blue card, nearly the same size, being put before him, he said it was blue and round. A triangular piece he also called round. The different colours of the objects placed before him he instantly decided on with great correctness, but had no idea of their form. He moved his eye to different distances, and seemed to see best at 6 or 7 inches. His focal distance has been since ascertained to be 7 inches. He was asked whether the object seemed to touch his eye, he said "No;" but when desired to say at what distance it was, he could not tell. These experiments were made in the theatre of the hospital, in which the operation was performed, before the surgeons and all the students. He was highly delighted with the pleasure of seeing, and said it was "so pretty," even when no object was before him, only the light upon his eye. The eye was covered, and

One eye couched at 7 years old.

Effects of the operation.

Sense of vision after the operation.

and he was put to bed, and told to keep himself quiet, but upon the house-surgeon going to him half an hour afterwards, his eye was found uncovered, and he was looking at his bed curtains, which were close drawn. The bandage was replaced, but so delighted was the boy with seeing, that he again immediately removed it. This circumstance distressed the house-surgeon, who had been directed to prevent him from looking at any thing till the next day, when the experiment was to be repeated. Finding that he could not enforce his instructions, he thought it most advisable to repeat the experiment about two hours after the operation. At first the boy called the different cards round; but upon being shown a square, and asked if he could find any corners to it, he was very desirous of touching it. This being refused, he examined it for some time, and said at last that he had found a corner, and then readily counted the four corners of the square; and afterwards when a triangle was shown him, he counted the corners in the same way; but in doing so his eye went along the edge from corner to corner, naming them as he went along.

Next day, when I saw him, he told me he had seen "the soldiers with their fifes and pretty things." The guards in the morning had marched past the hospital with their band; on hearing the music he had got out of bed, and gone to the window to look at them. Seeing the bright barrels of the musquets, he must in his mind have connected them with the sounds which he heard, and mistaken them for musical instruments. On examining the eye 24 hours after the operation, the pupil was found to be clear. A pair of scissors was shown him, and he said it was a knife. On being told he was wrong, he could not make them out; but the moment he touched them he said they were scissors, and seemed delighted with the discovery. On being shown a guinea at the distance of 15 inches from his eye, he said it was a seven shilling piece, but placing it about 5 inches from his eye, he knew it to be a guinea; and made the same mistake, as often as the experiment was repeated.

From this time he was constantly improving himself by looking at, and examining with his hands, every thing within his reach, but he frequently forgot what he had learnt.

On

On the 10th I saw him again, and I told him his eye was so well, that he might go about as he pleased without leaving the room. He immediately went to the window, and called out, "What is that moving?" I asked him what he thought it was? He said, "A dog drawing a wheelbarrow. There is one, two, three dogs drawing another. How very pretty!" These proved to be carts and horses on the road, which he saw from a two pair of stairs window.

Sense of vision after the operation.

On the 19th, the different coloured pieces of card were separately placed before his eye, and so little had he gained in thirteen days, that he could not without counting their corners one by one tell their shape. This he did with great facility, running his eye quickly along the outline, so that it was evident he was still learning, just as a child learns to read. He had got so far as to know the angles, when they were placed before him, and to count the number belonging to any one object.

The reason of his making so slow a progress was, that these figures had never been subjected to examination by touch, and were unlike any thing he was accustomed to see.

He had got so much the habit of assisting his eyes with his hands, that nothing but holding them could keep them from the object.

On the 26th the experiments were again repeated on the couched eye, to ascertain the degree of improvement which had been made. It was now found that the boy, on looking at any one of the cards in a good light, could tell the form nearly as readily as the colour.

From these two cases the following conclusions may be drawn:

General conclusions.

That, where the eye, before the cataract is removed, has only been capable of discerning light, without being able to distinguish colours, objects after its removal will seem to touch the eye, and there will be no knowledge of their outline; which confirms the observations made by Mr. Cheselden:

That where the eye has previously distinguished colours, there must also be an imperfect knowledge of distances, but not of outline, which however will afterwards be very soon acquired, as happened in Mr. Ware's cases. This is proved

proved by the history of the first boy in the present Paper, who before the operation had no knowledge of colours or distances, but after it, when his eye had only arrived at the same state, that the second boy's was in before the operation, he had learnt that the objects were at a distance, and of different colours: that when a child has acquired a new sense, nothing but great pain or absolute coercion will prevent him from making use of it.

Cataracts in children generally soft, and couching preferable to extraction.

In a practical view, these cases confirm every thing, that has been stated by Mr. Pott and Mr. Ware, in proof of cataracts in children being generally soft, and in favour of couching, as being the operation best adapted for removing them. They also lead us to a conclusion of no small importance, which has not before been adverted to; that, when the cataract has assumed a fluid form, the capsule, which is naturally a thin transparent membrane, has to resist the pressure of this fluid, which like every other diseased accumulation is liable to increase, and distend it, and therefore the capsule is rendered thicker and more opaque in its substance, like the coats of encysted tumours in general.

The earlier the operation is performed the better.

As such a change is liable to take place, the earlier the operation is performed in all children, who have cataracts completely formed, the greater is their chance of having distinct vision after the operation. It is unnecessary to point out the advantages to be derived from its being done at a more early age, independent of those respecting the operation itself.

VI.

Experiments on various Species of Cinchona: by Mr. VAUQUELIN*.

Several kinds of Peruvian bark in the shops.

The three chief.

The common:

SEVERAL different kinds of cinchona are met with in the shops, but the chief and most in use are the following three. First that formerly called by the vague name of Peruvian bark, and which appears to be taken from the cinchona officinalis L. This is externally of a gray colour, and internally

* Abridged from the Annales de Chimie, vol. LIX, p. 113, Aug. 1806.

nally of a pale red; thin, and convoluted from the contraction of the inner surface; smooth and as it were resinous in its fracture, but sometimes slightly fibrous; and of an astringent and bitter taste. Its powder is fawn coloured, mingled with a tinge of gray.

The second, known by the name of red bark, and sometimes erroneously called in France quinquina pitton, is of a much deeper colour; commonly very thick; little if at all convoluted; fibrous, and not at all resinous in its fracture; with an astringent and very slightly bitter taste. the red:

The third, or yellow bark, which is of most recent date, must not be confounded with the Angustura bark, as is sometimes done by the French druggists. This is of a pale yellow colour; of a more bitter but less astringent taste than either of the preceding; partly woody, partly resinous in its fracture; and a little convoluted, according as it is more or less thick. the yellow.

It would be of important service to the physician, as well as to the merchant, if there were any sure and simple methods of distinguishing the good kinds of cinchona from such as are bad or damaged: but hitherto we have nothing to guide us except their appearance, which may be fallacious, and our judgment from which must depend on our individual skill and practice. Mr. Seguin indeed has said, that the aqueous infusion of the good kinds possesses exclusively the property of precipitating infusion of tan, and that of the bad of precipitating animal gelatine; but this is an errour, for there are several species of true cinchona, that do not precipitate tannin, and yet cure fever*. No ready method of distinguishing their goodness. Seguin mistaken.

I have compared the physical and chemical properties of the infusions of every kind of cinchona to be found in the shops, to which I have added that of some other vegetable substances, apparently analogous with cinchona, and which are said to have cured fever. The infusions were prepared with the same quantity of water, the same quantity of bark, at an equal temperature, and for an equal time, so that no difference could arise from the mode of preparation.

* Our readers will recollect, that Seguin fancied he had discovered the febrifuge principle in cinchona to be nothing more or less than gelatine. See Journal, Vol. VI, p. 166.

SPEC.

SPEC. 1. *Yellow bark.*

Infusion of yellow bark.

122 grammes, or near 4 oz troy, of this bark, infused for twenty-four hours in two litres [a little more than 2 wine quarts] of water at 12° [54·6° F.], imparted to it a yellow colour, and a very bitter and slightly astringent taste.

Tested with various reagents.

This infusion occasioned a very copious flocculent precipitate in a solution of isinglass.

In a solution of sulphate of iron it produced a green colour resembling that of bile, and some time after a precipitate of the same colour fell down.

The solution of antimoniated tartrite of potash was precipitated by it of a yellowish white.

The oxalate of ammonia threw down from it a precipitate, which was oxalate of lime.

Lastly it very evidently reddened tincture of litmus.

This infusion, when completely precipitated by a solution of isinglass, and filtered, was deprived of colour, and scarcely at all astringent, but it retained its bitterness. In this state mixed with a solution of sulphate of iron, it turned it green as before, except that the colour inclined more to a yellow. It still precipitated the solution of emetic tartar, with this difference, that the precipitate was whiter. This cannot be ascribed to an excess of the isinglass, for a solution of isinglass occasions no change in that of emetic tartar.

Another portion of the infusion, being completely precipitated by emetic tartar and filtered, still rendered the solutions of isinglass and sulphate of iron turbid, but much less than before. The precipitate formed by the emetic tartar was turned slightly green by the addition of a few drops of sulphate of iron.

Principle that precipitates tartarised antimony different from that which precipitates gelatine.

It would appear from these experiments, that the principle which precipitates emetic tartar, isinglass, and sulphate of iron, is the same: and that, if the liquor still retain the property of precipitating isinglass and sulphate of iron, it is because it retains some portions of the combination of this principle with antimony. This supposition however is not reconcilable with the very copious precipitation of isinglass by certain kinds of cinchona, that do not precipitate emetic tartar. The principle that precipitates isinglass therefore must

must be different from that which decomposes tartarised antimony.

The bark left after this infusion being boiled in water, the decoction had almost exactly the same effects on the reagents above enumerated: the only difference between them was, that the decoction became turbid on cooling, furnished a smaller quantity of precipitate, and this separated from the liquor more speedily. Residuum decocted.

I have to add, that both of them threw down from the solution of sulphate of copper a reddish yellow precipitate, and from that of acetate of lead a yellowish white. With other reagents.

SPEC. 2. *Santa Fe bark.*

This bark, which is lately introduced, has been found to possess the febrifuge power by able physicians. It is gray on the outside, red within, thick, little convoluted, with an astringent and slightly bitter taste. Its infusion is much redder than that of the yellow bark. Tried in the same manner it produced the following effects. Santa Fe bark.

With the solution of isinglass it gives a very copious reddish flocculent precipitate. This effect, which has never yet been mentioned by any person to my knowledge, is worthy of remark. Its action with reagents.

It occasioned no change in solution of emetic tartar, in which it differs from the yellow bark.

It throws down a fine deep green precipitate from solution of sulphate of iron; perceptibly reddens tincture of litmus; is precipitated by oxalate of ammonia, but the oxalate of lime it thus yields is much less than that from the yellow bark.

It precipitates acetate of lead and sulphate of copper of a reddish brown.

The principle which precipitates emetic tartar appears to be wanting in this bark: and a farther proof of its differing in some respects from the yellow bark is, that their infusions on mixture become turbid. Its difference from yellow bark.

The decoction of this species produced the same effects with reagents as its infusion: but it is observable, that it does not grow turbid on cooling. Decoction.

SPEC.

SPEC. 3. *Gray bark, called superfine.*

Gray bark. The infusion of this species is nearly colourless. Its taste is bitter and astringent.

Its action with reagents. It forms a very copious white precipitate with isinglass, a red with infusion of tan, a copious and white with emetic tartar, and a very fine emerald green with sulphate of iron. It produces no change in infusion of yellow bark.

SPEC. 4. *Cinnamon gray bark.*

Cinnamon gray. The infusion is of a deep red, and has a bitter astringent taste.

Its action with reagents. It precipitates solution of isinglass of a brown fawn colour, gives a green colour with sulphate of iron, but does not precipitate emetic tartar.

It occasions no change in the infusion of the gray bark, but it produces a brown precipitate with that of the yellow bark, and does not precipitate the infusion of tan.

Gelatine and tartarised antimony precipitated by different principles. These vegetable infusions, after having precipitated each other as completely as possible, act no longer on emetic tartar: whence it follows, that the principle in the infusion of yellow bark which precipitates this salt combines with something in the infusion of cinnamon gray bark and tan. But these infusions, thus precipitated, still throw down an abundant precipitate from solution of isinglass, whence it follows, that these two substances are precipitated by different principles.

Precipitate from spec. 1 and 4. The precipitate formed by mixing the infusions of the 1st and 4th species dries easily, swells up when heated, gives out a smoke devoid of acrimony, and having some analogy to that of animal substances, and leaves a light spongy coal.

SPEC. 5. *Red bark, called pitton in the shops.*

Red bark. This is erroneously named, for the true pitton bark has different characters, as will be seen farther on.

Its infusion has a light orange red colour, and an astringent bitter taste.

Its action with reagents. It forms a copious reddish precipitate with isinglass, yellowish white with emetic tartar, brown with the infusion of the

the cinnamon gray bark, green with sulphate of iron. On the other metallic solutions it acts like other species of cinchona.

Spec. 6. *Gray bark*.

This, which I had from Mr. Bouillon Lagrange, was very thin and convoluted; and apparently from twigs or very young trees of the Loxa bark, which will be mentioned further on. Gray bark.

The infusion of this species had the red colour of Malaga wine, and an astringent bitter taste. It gave a copious white precipitate with isinglass, reddish yellow with infusion of tan, gray with infusion of yellow bark, yellowish white and flocculent with emetic tartar, green with sulphate of iron, white with acetate of lead. It did not precipitate sulphate of copper, or infusion of Santa Fe bark. It must possess the febrifuge property in a high degree. Its action with reagents. Highly febrifuge.

Spec. 7. *Flat gray bark**.

The infusion of this bark has the colour of Malaga wine, and a flat taste, without any astringency or bitterness. Flat gray bark.

From the infusion of yellow bark it throws down a copious, brown, flocculent precipitate. To the solution of red sulphate of iron it gives a fine green colour, and in a few minutes a precipitate of the same colour is thrown down. Neither tartarised antimony, isinglass, nor cinnamon gray bark produces any change in its infusion. Its action with reagents.

These appearances indicate, that it is not a true cinchona; or, if it belong to the genus, at least it has not its chemical properties; whence we may presume, that it does not possess the same medicinal virtues. Not a cinchona.

Spec. 8. *Yellow* [white] *bark, cinchona pubescens of Vahl.*

A hundred grammes of this bark in fine powder macerated four and twenty hours in distilled water afforded a transparent liquor of a golden yellow colour, very bitter, and frothing when White bark.

* This appears to be the white cinchona of Santa Fe brought over by Mr. von Humboldt, which will be noticed further on.

when shaken. With reagents it exhibited the following appearances.

Its action with reagents.

Tincture of galls formed in it a copious precipitate, which an excess of the tincture redissolved, and the addition of water again threw down. This shows, that the matter separated by the tannin is not purely animal.

From the solutions of tartarised antimony and nitrate of mercury it threw down a yellowish white precipitate. To that of sulphate of iron it gave a decided green colour, but nothing fell down. Solution of isinglass produced no change in it. It did not redden infusion of litmus.

Deposite from it.

During evaporation this infusion deposited a rosecoloured substance on the sides of the dish; and being reduced to the consistence of a sirup, it deposited farther on cooling a fresh quantity of a chesnut-brown substance. The filtered liquor was still coloured, and contained the salt peculiar to cinchonas, which will be noticed hereafter.

The brown substance, washed with a small quantity of cold water, is soluble in warm water and in alcohol; but very sparingly in cold water. Its taste is very bitter.

In the aqueous solution of this sediment nutgalls form a copious precipitate. Tartarised antimony and nitrate of mercury produce the same effects in this solution as in the infusion of the bark itself. Sulphate of iron is turned green by it. Oxigenized muriatic acid loses its smell when poured into the solution, and presently forms a flocculent precipitate. Isinglass has no effect on it: it is not changed by sulphuric or acetic acid: and when diluted with caustic potash it gives out no smell of ammonia.

Two hundred and twenty-five grammes [3475 grs.] of this substance, weighed when dry, afforded on distillation a great deal of water, a perceptible quantity of ammonia, and a purple oil, which loses this colour on being dissolved in alcohol, but resumes it as the menstruum evaporates by being left exposed to the air.

They left in the retort 11 decig. [17 grs.] of coal, which yielded by incineration 1 dec. [1·54 grs.] of ashes soluble with effervescence in muriatic acid, and the solution of which yielded lime and iron.

It

It is evident from what has been seen, that it is this coloured, bitter substance, which, in the maceration of the cinchona in question, produces with reagents all the phenomena mentioned above. This substance seems to be a medium, in its nature and properties, between vegetable and animal substances. Probably it is the efficacious principle in the cure of intermittent fevers. The liquor separated from this substance was treated with alcohol, which took up the colouring matter; and this proved to be nothing but a portion of the same substance, that the water had retained. The portion insoluble in alcohol was of the consistence of a thick mucilage, and had scarcely any taste or colour. It dissolved in large quantity in water; and the solution yielded by spontaneous evaporation slightly coloured and lamellated crystals of a salt, which will be farther noticed in the sequel.

This a mean between animal and vegetable matter.

Probably the febrifuge principle.

Salt of bark.

The same portion of cinchona, when it had been macerated for the seventh time, still giving a precipitate with galls, I conceived, that the cold water had been incapable of dissolving the whole of the principle, by which this effect was produced. In consequence I boiled the residuum of the cinchona, and the liquor thus obtained exhibited the same phenomena as the infusion, except that it did not precipitate the solution of tartarised antimony, probably because it was too much diluted with water.

Residuum decocted.

This bark therefore is not the same as species 1, though they are both called by the same name.

Spec. 9. *Common bark, cinchona officinalis.*

Eighty-four grammes [1297 grs.] of this bark, treated like the preceeding, afforded a paler coloured liquor, and more mucilaginous, though equally bitter. This infusion slightly reddened that of litmus. With other reagents it exhibited similar phenomena to the cinchona pubescens.

Common bark of the shops.

Its action with reagents.

All the liquors obtained by maceration when evaporated afforded a sediment, the properties of which so much resembled those of the same substance from the cinchona pubescens, that I conceived they might be mixed together: but the supernatant liquor, containing the salt essential to cinchona, was evaporated separately, and set to crystallize by sponta-

neous evaporation, after the colouring matter had been separated by alcohol, and in a few days crystals were produced from it.

Thus we have two species of cinchona, which do not precipitate isinglass, and which are consequently destitute of the principle, that produces this effect in other species. According to Mr. Seguin they are to be classed among the best sorts.

After several washings with cold water, as galls still occasioned a precipitate, the residuum was treated with hot water, which acquired a pretty deep colour. This was less bitter than the liquor obtained by maceration, and still more mucilaginous than the decoction of the cinchona pubescens. It formed a precipitate with galls and nitrate of mercury, and turned green with sulphate of iron; but neither tartarised antimony nor isinglass occasioned any change it it.

This species therefore is not the same with that examined above by the name of *gray*, and called *superfine*.

SPEC. 10. *Large-leaved bark, cinchona magnifolia.*

Large-leaved bark.

A hundred grammes of this bark in fine powder, macerated for twenty-four hours, yielded a solution that did not pass the filter easily. It was of a ruby red colour, little mucilaginous, slightly bitter, and very decidedly astringent.

Its action with reagents.

This infusion did not redden that of litmus: neither galls nor tartarised antimony afforded any precipitate with it: with solution of isinglass it gave a copious precipitate: sulphate of iron gave it the green hue of oxide of chrome, which muriatic acid converted into a dirty green. With the infusions of the eighth and ninth species it gave a precipitate.

The water in which it was steeped cold a second time did not precipitate isinglass.

The several waters in which it had been macerated were evaporated to the consistence of an extract, and treated with hot alcohol, which acquired from it a very fine colour. This alcoholic solution diluted with water, and tested with the reagents employed with the first water in which it had been macerated, exhibited the same results. The matter therefore, that produced the effects above enumerated, is soluble in alcohol.

The

The part not soluble in alcohol was of an ochre red, and blackened by exposure to the air. It was redissolvable in water; and its solution precipitated neither isinglass nor galls: but it precipitated tartarised antimony and nitrate of mercury, and turned sulphate of iron green. Portion not soluble in alcohol.

Ten grammes [154½ grs.] of this substance insoluble in alcohol being distilled afforded ammonia, and a coal that weighed 41 cent. [6⅓ grs.]

*A bark sold me without any name**.

A hundred grammes of this bark macerated for twenty-four hours did not give the water so deep a colour as the preceeding species, and its astringency was less, but it was more bitter. Another species analogous to the preceeding.

It perceptibly reddened infusion of litmus; precipitated neither with galls nor tartarised antimony; but formed a precipitate with isinglass and nitrate of mercury, and turned sulphate of iron green.

This species exhibited all the characters in general of the preceding, and should be placed in the same class.

The decoction of the residuum showed no difference from the infusion.

SPEC. 11. *True pitton bark.*

This species, which was given me by Mr. Solomé, an eminent apothecary in Paris, greatly resembles in colour, form, and bitterness the cinchona of St. Domingo, which was analysed by Mr. Fourcroy about fifteen years ago. Pitton bark.

A hundred grammes of this bark, treated like the other, imparted to the water a red colour like that of venous blood. Its taste is more bitter and disagreeable than that of the others. Tincture of galls, tartarised antimony, nitrate of mercury, and sulphate of iron, produced copious precipitates with this infusion of cinchona. Isinglass produced no change in it. It was precipitated by oxigenised muriatic acid, but by no other. Its action with reagents.

The infusion left by evaporation a residuum, which partly dissolved in alcohol, communicating to it a fine red colour.

* It had all the characters of the cinchona magnifolia [grandifolia].

The portion not soluble in alcohol had a gray colour and earthy appearance. It yielded ammonia on distillation. The portion dissolved exhibited the same phenomena as the infusion from which it was obtained.

Cinchona of different kinds brought from America by Messrs. von Humboldt and Bonpland.

Spec. 12. *Bark of Loxa, taken from branches of the second year, and used by the apothecary to the king of Spain.*

Loxa bark.

This is externally gray, internally yellow, thin, convoluted, and bitter and astringent to the taste.

Its action with reagents.

Eight grammes of this bark, infused for twenty-four hours in 150 grammes of water at 15° [59° F], yielded a reddish yellow liquor not very deeply coloured, having a slightly mouldy smell, and a bitter taste. It precipitated galls, tartarised antimony, and acetate of lead of a yellowish white, iron of a bluish green, oxalate of ammonia white, and isinglass in large, white, glutinous flocks. The precipitates formed by tartarised antimony and isinglass redissolved in an excess of the hot infusion.

Highly febrifuge.

From these properties this cinchona must have great febrifuge virtue.

Spec. 13. *White bark of Santa Fe.*

White bark of Santa Fe.

This bark has a rusty yellow colour externally, which is deeper within. It is flat and thick. Its fracture is granulated nearly like that of beech bark. Its taste is neither bitter nor astringent like that of the other barks.

Its action with reagents.

Eight grammes macerated for twenty-four hours in 150 grammes of water imparted to it a deeper yellow colour than the Loxa barks. This infusion precipitated neither galls, tartarised antimony, nor isinglass; it turned solution of iron green; and precipitated acetate of lead of a brownish yellow.

Not a cinchona.

From these properties this bark appears not to be a true cinchona.

Spec. 14. *Orange-coloured bark of Santa Fe.*

Orange coloured bark of Santa Fe.

This bark is of a cinnamon yellow colour, without any epidermis, thick, and very fibrous in its fracture. The thinnest

nest pieces are convoluted, the thickest flat. It is not at all astringent.

Its infusion, made as above, is scarcely coloured; has a decidedly bitter taste; forms a copious white precipitate with tartarised antimony; precipitates tannin, but not isinglass; turns iron slightly green; and does not render the infusion of Loxa bark turbid. This species of cinchona differs from that of Loxa, and cannot have very striking febrifuge properties. Its action with reagents. Of little virtue.

Spec. 15. *Common peruvian bark.*

This bark is gray externally, and of an ochre red within: its surface is wrinkled; it is convoluted, and of various thicknesses according to the difference of the pieces; its taste is bitter and astringent. Common bark.

Eight grammes, macerated for twenty-four hours in 150 grammes of water, gave it a light yellow colour, and a bitter and astringent taste. This infusion precipitated tartarised antimony, isinglass, and tannin of a yellowish white, and sulphate of iron green. It reddened litmus paper. Its action with reagents.

This bark appears to be the same with the gray, called superfine, spec. 3. From the properties it exhibited it must be excellent in fevers, &c. An excellent febrifuge.

Spec. 16. *Red bark of Santa Fe.*

This does not appear to differ in any sensible degree from that mentioned above by the name of Santa Fe bark, spec. 2. Red bark of Sante Fe.

Eight grammes, macerated as above, gave an infusion of the red colour of Malaga wine, with an astringent taste, and but little bitterness. It precipitated isinglass brown; gave no precipitate with tannin or tartarised antimony; turned sulphate of iron green; and slightly reddened litmus paper. These chemical properties are equally apparent in the Santa Fe bark described above. Its action with reagents.

Spec. 17. *Yellow bark of Cuença, from branches of four or six years old.*

This bark is gray exteriorly, covered with a white lichen, of a brown yellow interiorly, having a fibrous fracture, and scarcely any taste. Its infusion is neither bitter nor astringent. Yellow bark of Cuença.

gent. It precipitates neither tartarised antimony, isinglass, nor tannin; merely turns sulphate of iron green; but precipitates acetate of lead.

Not febrifuge. It can have no febrifuge virtue.

Table of the properties of the barks brought over by von Humboldt.

Table of the effects produced by the species of cinchona brought from America by Messrs. von Humboldt and Bompland with the reagents mentioned.

SPECIES.	ISINGLASS.	TANNIN.	IRON.	TART. ANTIM.	OBSERVATIONS.
1 Common gray peruvian bark.	Copious precipitate	The same.	A green colour.	Copious precipitate.	A bitter, astringent taste. Reddens litmus.
2 Red bark of Santa Fe.	Copious precipitate.	None.	A green colour.	None.	Red colour of Malaga wine: little bitterness, but astringent to the taste.
3 Yellow bark of Cuenca.	None.	None.	Greened.	None.	Neither bitter nor astringent. Precipitates acetate of lead.
4 King of Spain's Loxa bark.	Copious precipitate.	The same.	Green.	Copious precipitate.	Reddish colour, not deep. Mouldy smell. Bitter.
5 White bark of Santa Fe.	None.	None.	Greened.	None.	Pretty deep yellow colour. Neither bitter nor astringent. Precipitates acetate of lead.
6 Yellow bark of Santa Fe.	None.	Copious precipitate	Greened.	Copious precipitate.	Taste decidedly bitter, slightly astringent. Infusion light coloured.

To

To gain some additional light respecting the nature of the principles contained in cinchona, I instituted a comparative examination of several other vegetable substances, that appear to have some analogy with it, and the composition of which is a little better known; such as galls, oak, bark, Angustura bark, and some others. Other substances compared with cinchona.

Nutgalls.

The infusion of this substance copiously precipitated isinglass white; iron, blue; tartarised antimony, yellowish white; infusion of yellow bark, in dirty white flocks; copper, brown yellow; and lead, yellowish white. Galls.

It did not precipitate infusion of Santa Fe bark, or of tan.

The infusion of nutgalls therefore, like that of yellow bark, contains the principle that precipitates isinglass with that which precipitates tartarised antimony; and in this respect they resemble each other. But they differ with regard to the principle that acts on tan and on iron, since their metal is precipitated green by cinchona, and blue by galls. They differ too in another point, since they mutually precipitate each other.

Tan.

The infusion of this substance, made with the same care and in the same proportions as those of the cinchona bark, precipitated solution of isinglass yellowish; iron blue; copper, brown: but it occasioned no change in solution of Santa Fe bark, or solution of tartarised antimony. It reddened infusion of litmus, and was precipitated by oxalate of ammonia. Oak bark.

Hence we see, that oak bark does not contain the substance that precipitates tartarised antimony, as nutgalls, yellow bark, and some other barks do; and in this respect it differs from them, though they agree in precipitating isinglass.

Cherry-tree bark.

This bark, which has sometimes been fraudulently substituted for that of cinchona, has nothing in common with it Bark of the cherry-tree.

it except the property of forming a green precipitate with solution of sulphate of iron. It occasions no change in isinglass, tartarised antimony, or decoction of oak bark. Its possessing any febrifuge property therefore is very questionable.

Centaury and Germander.

Centaurea and chamædrys.

These two plants afforded me the same results as cherry-tree bark: their efficacy in fever therefore is equally doubtful.

White willow bark.

Bark of the white willow.

This bark, to which febrifuge virtues have formerly been ascribed, possesses in fact some of the chemical properties of certain species of cinchona, namely those of precipitating isinglass, and throwing down sulphate of iron green, and acetate of copper brownish. The white willow bark, therefore, as it unites the bitter and astringent tastes, may possibly be a febrifuge.

Angustura bark.

Angustura bark.

The infusion of this bark does not precipitate isinglass: but it forms a copious precipitate with infusion of nutgalls, and with that of yellow bark, though it merely renders infusion of Santa Fe bark slightly turbid.

It precipitates iron, tartarised antimony, copper, lead, and infusion of tan, all yellow.

This bark, we see, differs from several of the species of cinchona, and from the other substances submitted to the comparative examination, in not precipitating animal gelatine. It wants too the astringent taste, but on the other hand is extremely bitter. There is reason to believe too, that the principle, which in this precipitates the metallic solutions, is not altogether the same with that in the cinchonas: at least the colour of the precipitates it gives is very different. From these properties however the Angustura bark may possibly be a febrifuge.

(To be concluded in the next number.)

Experiments

VII.

Experiments for investigating the Cause of the coloured concentric Rings, discovered by Sir ISAAC NEWTON, *between two Object-glasses laid upon one another. By* WILLIAM HERSCHEL, *LL.D. F.R.S.**

Coloured rings, Sir I. Newton supposed, may lead to a completion of the theory of light.

THE account given by Sir I. Newton of the coloured arcs and rings, which he discovered by laying two prisms or object-glasses upon each other, is highly interesting. He very justly remarks, that these phenomena are "of difficult consideration," but that "they may conduce to farther discoveries for completing the theory of light, especially as to the constitution of the parts of natural bodies on which their colours or transparency depend†."

His explanation of them unsatisfactory.

With regard to the explanation of the appearance of these coloured rings, which is given by Sir I. Newton, I must confess, that it has never been satisfactory to me. He accounts for the production of the rings, by ascribing to the rays of light certain fits of easy reflection and easy transmission alternately returning and taking place with each ray at certain stated intervals‡. But this, without mentioning particular objections, seems to be an hypothesis, which cannot be easily reconciled with the minuteness and extreme velocity of the particles, of which these rays, according to the Newtonian theory, are composed.

Dr Herschel has pursued the subject to some extent,

The great beauty of the coloured rings, and the pleasing appearances arising from the different degrees of pressure of the two surfaces of the glasses against each other when they are formed, and especially the importance of the subject, have often excited my desire of inquiring farther into the cause of such interesting phenomena; and with a view to examine them properly I obtained, in the year 1792, the two object-glasses of Huygens, in the possession of the Royal Society, one of 122, and the other of 170 feet focal length, and began a series of experiments with them, which, though

* From the Phil. Trans. for 1807, Part III, p 180.

† Newton's Optics, 4th ed. p. 169.　　‡ Ibid. p. 256.

many

many times interrupted by astronomical pursuits, has often been taken up again, and has lately been carried to a very considerable extent. The conclusions that may be drawn from them, though they may not perfectly account for all the phænomena of the rings, are yet sufficiently well supported, and of such a nature as to point out several modifications of light that have been totally overlooked, and others that have never been properly discriminated. It will, therefore, be the aim of this paper to arrange and distinguish the various modifications of light in a clear and perspicuous order, and afterwards to give my sentiments upon the cause of the formation of the concentric rings. The avowed intricacy of the subject*, however, requires, in the first place, a minute detail of experiments, and afterwards a very gradual developement of the consequences to be deduced from them.

His experiments led to new conclusions, and discriminations.

Minute detail necessary.

Term modification.

As the word modification will frequently be used, it may not be amiss to say, that when applied to light, it is intended to stand for a general expression of all the changes that are made in its colours, direction, or motion: thus, by the modification of reflection, light is thrown back; by that of refraction, it is bent from its former course; by the modification of dispersion, it is divided into colours, and so of the rest.

I. *Of different Methods to make one set of concentric Rings visible.*

One set of rings made visible by different methods.

In the beginning of my experiments I followed the Newtonian example, and, having laid the two object-glasses of Huygens upon one another, I soon perceived the concentric rings. It is almost needless to say, that I found all the Newtonian observations of these rings completely verified; but as his experiments seemed to be too much confined for drawing general conclusions, I endeavoured to extend them: and by way of rendering the methods I point out very clear, I have given one easy particular instance of each, with the addition of a generalization of it, as follows:

1st method. Double convex lens on a plane of glass.

First Method. On a table placed before a window I laid down a slip of glass, the sides of which were perfectly plain, parallel, and highly polished. Upon this I laid a double

* Newton's Optics, 4th ed. p. 283; end of Obs. 13.

convex

convex lens of 26 inches focal length, and found that this arrangement gave me a set of beautiful concentric rings.

I viewed them with a double convex eye lens of 2½ inches focus mounted upon an adjustable stand, by which simple apparatus I could examine them with great ease; and as it was not material to my present purpose by what obliquity of incidence of light I saw the rings, I received the rays from the window most conveniently when they fell upon the lens in an angle of about 30 degrees from the perpendicular, the eye being placed on the opposite side at an equal angle of elevation to receive the reflected rays.

Generalization. Instead of a plain slip of glass, the plain side of a plano-concave, or plano-convex lens of any focal length whatsoever may be used: and when the convex side of any lens is laid upon it, whatever may be the figure of the other surface, whether plain, concave, or convex, and whatever may be its focal length, a set of concentric rings will always be obtained. I have seen rings with lenses of all varieties of focus, from 170 feet down to one quarter of an inch. Even a common watch glass laid upon the same plain surface will give them. Generalization.

To insure success, it is necessary, that the glasses should be perfectly well cleaned from any adhering dust or soil, especially about the point of contact; and in laying them upon each other a little pressure should be used, accompanied at first with a little side motion, after which they must be left at rest. Necessary precautions.

If the surface of the incumbent lens, especially when it is of a very short focal length, is free from all imperfection and highly polished, the adjustment of the focus of the above mentioned eye-glass, which I always use for viewing the rings, is rather troublesome, in which case a small spot of ink made upon the lens will serve as an object for a sufficient adjustment to find the rings.

Second Method. Instead of the slip of glass, I laid down a well polished plain metalline mirror; and placing upon it the same 26-inch double convex lens, I saw again a complete set of concentric rings. 2d method. Double convex lens on a metallic mirror.

It is singular that, in this case, the rings reflected from a bright metalline surface will appear fainter than when the

same

same lens is laid on a surface of glass reflecting but little light; this may however be accounted for by the brilliancy of the metalline ground, on which these faint rings are seen, the contrast of which will offuscate their feeble appearance.

Generalization.

Generalization. On the same metalline surface every variety of lenses may be laid, whatever be the figure of their upper surface, whether plain, concave, or convex, and whatever be their focal lengths, provided the lowest surface remains convex, and concentric rings will always be obtained; but for the reason mentioned in the preceding paragraph, very small lenses should not be used, till the experimentalist has been familiarized with the method of seeing these rings, after which lenses of two inches focus, and gradually less, may be tried.

3d. method. Double convex lens on a plano convex.

Third Method. Hitherto we have only used a plain surface, upon which many sorts of glasses have been placed; in order therefore to obtain a still greater variety, I have laid down a plano-convex lens of 15 inches focal length, and upon the convex surface of it I placed the 26-inch double convex lens, which produced a complete set of rings.

4th. The same on convex metal.

Fourth Method. The same lens, placed upon a convex metalline mirror of about 15 inches focal length, gave also a complete set of rings.

Generalization.

Generalization. These two cases admit of a much greater variety than the first and second methods; for here the incumbent glass may have not only one, but both its surfaces of any figure whatsoever; whether plain, concave, or convex; provided the radius of concavity, when concave lenses are laid upon the convex surface of glass or metal, is greater than that of the convexity on which they are laid.

The figure of the lowest surface of the subjacent substance, when it is glass, may also be plain, concave, or convex; and the curvature of its upper surface, as well as of the mirror, may be such as to give them any focal length, provided the radius of their convexities is less than that of the concavity of an incumbent lens; in all which cases complete sets of concentric rings will be obtained.

5th. Double convex lens in a double concave glass.

Fifth Method. Into the concavity of a double concave glass of 8 inches focal length I placed a 7-inch double convex lens, and saw a very beautiful set of rings.

Sixth

Sixth Method. Upon a 7 feet concave metalline mirror I placed the double convex 26-inch lens, and had a very fine set of rings.

Generalization. With these two last methods, whatever may be the radius of the concavity of the subjacent surface, provided it be greater than that of the convexity of the incumbent glass; and whatever may be the figure of the upper surface of the lenses, that are placed upon the former, there will be produced concentric rings. The figure of the lowest surface of the subjacent glass may also be varied at pleasure, and still concentric rings will be obtained. 6th. Lens in concave metal.

II. *Of seeing Rings by Transmission.*

The great variety of the different combinations of these differently figured glasses and mirrors will still admit of farther addition, by using a different way of viewing the rings. Hitherto the arrangement of the apparatus has been such, as to make them visible only by reflection, which is evident, because all the experiments that have been pointed out may be made by the light of a candle placed so, that the angle of incidence and of reflection towards the eye of the observer may be equal. But Sir I. Newton has given us also an observation, where he saw these rings by transmission, in consequence of which I have again multiplied and varied the method of producing them that way, as follows: Rings by transmission.

First Method. On a slip of plain glass highly polished on both sides place the same double convex lens of 26-inches, which had already been used when the rings were seen by reflection. Take them both up together and hold them against the light of a window, in which position the concentric rings will be seen with great ease by transmitted light. But as the use of an eye-glass will not be convenient in this situation, it will be necessary to put on a pair of spectacles with glasses of 5, 6, or 7 inches focus, to magnify the rings in order to see them more readily. 1st method. Double convex lens on plain glass.

Second Method. It would be easy to construct an apparatus for viewing the rings by transmisson fitted with a proper eye-glass; but other methods of effecting the same purpose are preferable. Thus, if the two glasses that are to give the rings be laid upon a hollow stand, a candle placed at a pro- 2d. The same with the light from a candle below.

per

per angle and distance under them will show the rings conveniently by transmitted light, while the observer and the apparatus remain in the same situation as if they were to be seen by reflection.

3d. Daylight reflected upward from a mirror.

Third Method. A still more eligible way is to use daylight received upon a plain metalline mirror reflecting it upwards to the glasses placed over it, as practised in the construction of the common double microscope; but I forbear entering into a farther detail of this last and most useful way of seeing rings by transmission, as I shall soon have occasion to say more on the same subject.

Generalization.

Generalization. Every combination of glasses, that has been explained in the first, third, and fifth methods of seeing rings by reflection, will also give them by transmission, when exposed to the light in any of the three ways that have now been pointed out. When these are added to the former, it will be allowed, that we have an extensive variety of arrangements for every desirable purpose of making experiments upon rings, as far as single sets of them are concerned.

III. *Of Shadows.*

Of shadows.

When two or more sets of rings are to be seen, it will require some artificial means, not only to examine them critically, but even to perceive them; and here the shadow of some slender opaque body will be of eminent service. To cast shadows of a proper size and upon places where they are wanted, a pointed penknife may be used as follows.

Point of a penknife

gives two shadows from a plain or convex glass:

When a plain slip of glass or convex lens is laid down, and the point of a penknife is brought over either of them, it will cast two shadows, one of which may be seen on the first surface of the glass or lens, and the other on the lowest.

three from two glasses,

and in some cases four.

When two slips of glass are laid upon each other, or a convex lens upon one slip, so that both are in contact, the penknife will give three shadows; but if the convex lens should be of a very short focus, or the slips of glass be a little separated, four of them may be perceived; for in that case there will be one formed on the lowest surface of the incumbent glass or lens; but in my distinction of shadows this will not be noticed. Of the three shadows thus formed the second will

will be darker than the first, but the third will be faint. When a piece of looking glass is substituted for the lowest slip, the third shadow will be the strongest.

Three slips of glass in contact, or two slips with a lens upon them, or also a looking glass, a slip and a lens put together, will give four shadows, one from each upper surface and one from the bottom of the lowest of them. Four from three glasses.

In all these cases a metalline mirror may be laid under the same arrangement without adding to the number of shadows, its effect being only to render them more intense and distinct. Metallic mirror renders them plainer.

The shadows may be distinguished by the following method. When the point of the penknife is made to touch the surface of the uppermost glass or lens, it will touch the point of its own shadow, which may thus at any time be easily ascertained: and this in all cases I call the first shadow; that which is next to it, the second; after which follows the third, and so on. Method of distinguishing the shadows.

In receding from the point, the shadows will mix together, and thus become more intense; but which, or how many of them are united togther, may always be known by the points of the shadows. Mixture of the shadows.

When a shadow is to be thrown upon any required place, hold the penknife nearly half an inch above the glasses, and advance its edge foremost gradually towards the incident light. The front should be held a little downwards to keep the light from the underside of the penknife, and the shadows to be used should be obtained from a narrow part of it. Precautions.

With this preparatory information it will be easy to point out the use that is to be made of the shadows when they are wanted.

IV. *Of two sets of Rings.*

I shall now proceed to describe a somewhat more complicated way of observation, by which two complete sets of concentric rings may be seen at once. The new or additional set will furnish us with an opportunity of examining rings in situations where they have never been seen before, which will be of eminent service for investigating the cause of their origin, and with the assistance of the shadows to be formed, Two sets of rings.

formed, as has been explained, we shall not find it difficult to see them in these situations.

1st method. Double convex lens on looking glass:

First Method. Upon a well polished piece of good looking glass lay down a double convex lens of about 20 inches focus. When the eye-glass has been adjusted as equal for seeing one set of rings, make the shadow of the penknife in the order which has been described, pass over the lens; then as it sometimes happens in this arrangement that no rings are easily to be seen, the shadow will, in its passage over the surface, show where they are situated. When a set of them is perceived, which is generally the primary one, bring the third shadow of the penknife over it, in which situation it will be seen to the greatest advantage.

Secondary set of rings.

Then, if at the same time a secondary set of rings has not yet been discovered, it will certainly be perceived when the second shadow of the penknife is brought upon the primary set. As soon as it has been found out, the compound shadow, consisting of all the three shadows united, may then be thrown upon this secondary set, in order to view it at leisure and in perfection. But this compound shadow should be taken no farther from the point than is necessary to cover it; nor should the third shadow touch the primary set. The two sets are so near together, that many of the rings of one set intersect some of the other.

Viewed alternately with the primary.

When a sight of the secondary set has been once obtained, it will be very easy to view it alternately with the primary one by a slight motion of the penknife, so as to make the third shadow of it go from one set to the other.

The rings made visible by setting them in motion.

Besides the use of the shadows, there is another way to make rings visible when they cannot be easily perceived, which is to take hold of the lens with both hands, to press it alternately a little more with one than with the other; a tilting motion, given to the lens in this manner, will move the two sets of rings from side to side; and as it is well known that a faint object in motion may be sooner perceived than when it is at rest, both sets of rings will by these means be generally detected together.

The light should be oblique.

It will also contribute much to facilitate the method of seeing two sets of rings, if we receive the light in a more oblique angle of incidence, such as 40, 50, or even 60 degrees

grees. This will increase the distance between the centres of the primary and secondary sets, and at the same time occasion a more copious reflection of light.

Instead of a common looking-glass a convex glass mirror may be used, on which may be placed either a plain, a concave, or a convex surface of any lens or glass, and two sets of rings will be obtained. With glasses of other forms.

In the same manner, by laying upon a concave glass mirror a convex lens, we shall also have two sets of rings.

The generalizations that have been mentioned when one set of rings was proposed to be obtained may be easily applied with proper regulations, according to the circumstances of the case, not only to the method by glass mirrors already mentioned, but likewise to all those that follow hereafter, and need not be particularized for the future. In the choice of the surfaces to be joined, we have only to select such as will form a central contact, the focal length of the lenses and the figure of the upper surface being variable at pleasure. Generalization.

Second Method. On a plain metalline mirror I laid a parallel slip of glass, and placed upon it the convex surface of a 17-inch plano-convex lens, by which means two sets of rings were produced. 2d. Lens on glass and metal.

Upon the same mirror the plain side of the plano-convex glass may be laid instead of the plain slip, and any plain, convex, or concave surface, being placed upon the convexity of the subjacent lens, will give two sets of rings.

The plain side of a plano-concave glass may also be placed upon the same mirror, and into the concavity may be laid any lens that will make a central contact with it, by which arrangement two sets of rings will be obtained.

Third Method. Upon a small well polished slip of glass place another slip of the same size, and upon them lay a 39-inch double convex lens. This will produce two sets of rings; one of them reflected from the upper surface of the first slip of glass, and the other from that of the second. 3d. Lens on two slips of glass.

Instead of the uppermost plain slip of glass we may place upon the lowest slip the plain side of a plano-convex or plano-concave lens, and the same variety which has been explained in the third method, by using any incumbent lens that

will make a central contact, either with the convexity or concavity of the subjacent glass, will always produce two sets of rings.

4th. Lens on glass on black paper.

Fourth Method. A more refined but rather more difficult way of seeing two sets of rings is to lay a plain slip of glass on a piece of black paper, and when a convex lens is placed upon the slip, there may be perceived, but not without particular attention, not only the first set, which has already been pointed out as reflected from the first surface of the slip, but also a faint secondary set from the lowest surface of the same slip of glass.

It will be less difficult to see two sets of rings by a reflection from both surfaces of the same glass, if we use, for instance, a double concave of 8 inches focus with a double convex of $7\frac{1}{2}$ inches placed upon it. For, as it is well known that glass will reflect more light from the farthest surface when air rather than a denser medium is in contact with it, the hollow space of the 8-inch concave will give a pretty strong reflection of the secondary set.

5th. Two primary and independent sets of rings.

Fifth Method. The use that is intended to be made of two sets of rings requires, that one of them should be dependent upon the other: this is a circumstance that will be explained hereafter, but the following instance, where two independent sets of rings are given, will partly anticipate the subject. When a double convex lens of 50 inches is laid down with a slip of glass placed upon it, and another double convex one of 26 inches is then placed upon the slip, we get two sets of rings of different sizes; the large rings are from the 50-inch glass, the small rings from the 26-inch one. They are to be seen with great ease, because they are each of them primary.

These may be crossed and varied.

By tilting the incumbent lens, or the slip of glass, these two sets of rings may be made to cross each other in any direction; the small set may be laid upon the large one, or either of them may be separately removed towards any part of the glass. This will be sufficient to show, that they have no connection with each other. The phenomena of the motions, and of the various colours and sizes assumed by these rings, when different pressures and tiltings of the glasses are used, will afford some entertainment. With the assistance of the shadow

shadow of the penknife the secondary set belonging to the rings from the 26-inch lens will be added to the other two sets; but in tilting the glasses this set will never leave its primary one, while that from the 50-inch lens may be made to go any where across the other two.

V. *Of three sets of Rings.*

To see three sets of concentric rings at once is attended with some difficulty, but by the assistance of the methods of tilting the glasses, and making use of the multiplied shadows of a penknife, we may see them very well, when there is a sufficient illumination of bright daylight. Three sets of rings.

First Method. A 26-inch double convex lens placed upon three slips of plain glass will give three sets of rings. The slips of glass should be nearly 2 tenths of an inch thick, otherwise the different sets will not be sufficiently separated. When all the glasses are in full contact, the first and second sets may be seen with a little pressure and a small motion, and, if circumstances are favourable, the third, which is the faintest, will also appear. If it cannot be seen, some of the compound shadows of the penknife must be thrown upon it; for in this case there will be five shadows visible, several of which will fall together, and give different intensity to their mixture. 1st. A lens on three slips of glass.

Second Method. When a single slip of glass, with a 34-inch lens upon it, is placed upon a piece of good looking glass, three sets of rings may be seen: the first and third sets are pretty bright, and will be perceived by only pressing the lens a little upon the slip of glass; after which it will be easy to find the second set with the assistance of the proper shadow. In this case four shadows will be seen; and when the third shadow is upon the first set, the fourth will be over the second set and render it visible. 2d. A lens and a slip of glass on looking glass.

Third Method. When two slips of glass are laid upon a plain metalline mirror, then a 26-inch lens placed upon the slips will produce three sets of rings; but it is not very easy to perceive them. By a tilting motion the third set will generally appear like a small white circle, which at a proper distance will follow the movement of the first set. As soon as the first and third sets are in view, the third shadow of the pen- 3d. Lens on two glasses and metal.

 knife

knife may be brought over the first set, by which means the fourth shadow will come upon the second set, and in this position of the apparatus it will become visible.

4th. Lens on a slip of glass forming an angle with metal.

Fourth Method. On a plain metalline mirror lay one slip of glass, but with a small piece of wood at one end under it, so that it may be kept about one tenth of an inch from the mirror, and form an inclined plane. A 26-inch lens laid upon the slip of glass will give three sets of rings. Two of them will easily be seen; and when the shadow of the penknife is held between them the third set will also be perceived. There is but one shadow visible in this arrangement, which is the third; the first and second shadows being lost in the bright reflection from the mirror.

5th. A convex lens on a concave and slip of glass.

Fifth Method. I placed a 6¼-inch double convex upon an 8-inch double concave, and laid both together upon a plain slip of glass. This arrangement gave three sets of rings. They may be seen without the assistance of shadows, by using only pressure and tilting. The first had a black and the other two had white centres.

VI. *Of four sets of Rings.*

Four sets of rings.

The difficulty of seeing many sets of rings increases with their number, yet by a proper attention to the directions that are given four sets of concentric rings may be seen.

1st. Lens on a glass forming an angle with a mirror.

First Method. Let a slip of glass, with a 26-inch lens laid upon it, be placed upon a piece of looking glass. Under one end of the slip, a small piece of wood one tenth of an inch thick must be put, to keep it from touching the looking glass. This arrangement will give us four sets of rings. The first, third, and fourth may easily be seen, but the second set will require some management. Of the three shadows, which this apparatus gives, the second and third must be brought between the first and fourth sets of rings, in which situation the second set of rings will become visible.

2d. Plano-convex lens on three slips of glass & metal.

Second Method. When three slips of glass are laid upon a metalline mirror, and a plano-convex lens of about 17 inches focus is placed with its convex side upon them, four sets of rings may be seen; but this experiment requires a very bright day, and very clean, highly polished slips of plain

plain glass. Nor can it be successful unless all the foregoing methods of seeing multiplied sets of rings are become familiar and easy.

I have seen occasionally, not only four and five, but even six sets of concentric rings, from a very simple arrangement of glasses: they arise from reiterated internal reflections; but it will not be necessary to carry this account of seeing multiplied sets of rings to a greater length. 5 or 6 sets of rings.

VII. *Of the Size of the Rings.*

The diameter of the concentric rings depends upon the radius of the curvature of the surfaces between which they are formed. Curvatures of a short radius, *cæteris paribus*, give smaller rings than those of a longer; but Sir I. Newton having already treated on this part of the subject at large, it will not be necessary to enter farther into it. Size of the rings.

I should however remark, that, when two curves are concerned, it is the application of them to each other, that will determine the size of the rings, so that large ones may be produced from curvatures of a very short radius. A double convex lens of $2\frac{1}{4}$-inches focus, for instance, when it is laid upon a double concave which is but little more in focal length, gives rings that are larger than those from a lens of 26 inches laid upon a plain slip of glass. Inversely as the angle of contact.

VIII. *Of Contact.*

The size of the rings is considerably affected by pressure. They grow larger when the two surfaces that form them are pressed closer together, and diminish when the pressure is gradually removed. The smallest ring of a set may be increased by this means to double and treble its former diameter; but as the common or natural pressure of glasses laid upon any flat or curved surface is occasioned by their weight, the variations of pressure will not be very considerable, when they are left to assume their own distance or contact. To produce that situation, however, which is generally called contact, it will always be necessary, to give a little motion backwards and forwards to the incumbent lens or glass, accompanied with some moderate pressure, after which it may be left to settle properly by its own weight. Pressing the surfaces together enlarges the rings.

IX.

IX. *Of measuring Rings.*

The rings difficult to measure absolutely.

It may be supposed from what has been said concerning the kind of contact, which is required for glasses to produce rings, that an attempt to take absolute measures must be liable to great inaccuracy. This was fully proved to me, when I wanted to ascertain, in the year 1792, whether a lens laid upon a metalline surface would give rings of an equal diameter with those it gave when placed on glass. The measures differed so much, that I was at first deceived; but on proper consideration it appeared, that the Huygenian object glass, of 122 feet focus, which I used for the experiment, could not so easily be brought to the same contact on metal as on glass; nor can we ever be well assured, that an equal distance between the two surfaces in both cases has been actually obtained. The colour of the central point, as will be shown hereafter, may serve as a direction; but even that cannot be easily made equal in both cases. By taking a sufficient number of measures of any given ring of a set, when a glass of a sufficient focal length is used, we may however determine its diameter to about the 25th or 30th part of its dimension.

But their proportions in the same set more easily measured.

Relative measures, for ascertaining the proportion of the different rings in the same set to each other, may be more accurately taken, for in that case the contact with them all will remain the same, if we do not disturb the glasses during the time of measuring.

X. *Of the Number of Rings.*

Number of rings.

When there is a sufficient illumination, many concentric rings in every set will be perceived; in the primary set we see generally 8, 9, or 10, very conveniently. By holding the eye in the most favourable situation I have often counted near 20, and the number of them is generally lost, when they grow too narrow and minute to be perceived, so that we can never be said fairly to have counted them to their full extent. In the second set I have seen as many as in the first, and they are full as bright. The third set, when it is seen by a metalline mirror under two slips, will be brighter than the second, and almost as bright as the first: I have easily counted 7, 8, and 9 rings.

XI.

XI. *Of the Effect of Pressure on the Colour of the Rings.*

Their colours affected by pressure.

When a double convex object glass of 14 or 15 feet focus is laid on a plain slip of glass, the first colours that make their faintest appearance will be red surrounded by green; the smallest pressure will turn the centre into green surrounded by red: an additional pressure will give a red centre again, and so on till there have been so many successive alterations, as to give us six or seven times a red centre, after which the greatest pressure will only produce a very large black one surrounded by white.

When the rings are seen by transmission, the colours are in the same manner subject to a gradual alternate change occasioned by pressure, but when that is carried to its full extent, the centre of the rings will be a large white spot surrounded by black.

The succession and addition of the other prismatic colours, after the first or second change, in both cases is extremely beautiful; but as the experiment may be so easily made, a description, which certainly would fall short of an actual view of these phenomena, will not be necessary.

When the rings are produced by curves of a very short radius, and the incumbent lens is in full contact with the slip of glass, they will be alternately black and white; but by lessening the contact, I have seen, even with a double convex lens of no more than two tenths of an inch focus, the centre of the rings white, red, green, yellow, and black, at pleasure. In this case I used an eye-glass of one inch focus; but as it requires much practice to manage such small glasses, the experiment may be more conveniently made by placing a double convex lens of 2 inches focus on a plain slip of glass, and viewing the rings by an eye-glass of $2\frac{1}{2}$ inches; then having first brought the lens into full contact, the rings will be only black and white, but by gently lifting up or tilting the lens, the centre of the rings will assume various colours at pleasure.

XII. *Of diluting and concentrating the Colours.*

Method of diluting or con-

Lifting up or tilting a lens being subject to great uncertainty, a surer way of acting upon the colours of the rings is by

centrating the colours.

by dilution and concentration. After having seen that very small lenses give only black and white when in full contact, we may gradually take others of a longer focus. With a double convex lens of four inches the outward rings will begin to assume a faint red colour. With 5, 6, and 7, this appearance will increase; and proceeding with lenses of a larger focus, when we come to about 16, 18, or 20 inches, green rings will gradually make their appearance.

This and other colours come on much sooner if the centre of the lens is not kept in a black contact, which in these experiments must be attended to.

A lens of 26 inches not only shows black, white, red, and green rings, but the central black begins already to be diluted so as to incline to violet, indigo, or blue. With one of 34, the white about the dark centre begins to be diluted, and shows a kind of gray inclining to yellow. With 42 and 48, yellow rings begin to become visible. With 55 and 59, blue rings show themselves very plainly. With a focal length of 9 and 11 feet, orange may be distinguished from the yellow and indigo from the blue. With 14 feet, some violet becomes visible. When the 122-feet Huygenian glass is laid on a plain slip, and well settled upon it, the central colour is then sufficiently diluted, to show that the dark spot, which in small lenses, when concentrated, had the appearance of black, is now drawn out into violet, indigo, and blue, with a little admixture of green; and that the white ring, which used to be about the central spot, is turned partly green with a surrounding yellow, orange, and red-coloured space or ring; by which means we seem to have a fair analysis of our former compound black and white centre.

Analysis of the black & white centre.

One of my slips of glass, which is probably a little concave, gave the rings still larger, when the 122 feet glass was firmly pressed against it. I used a little side motion at the same time, and brought the glasses into such contact, that they adhered sufficiently to be lifted up together. With this adhesion I perceived a colour surrounding a dark centre, which I have never seen in any prismatic spectrum. It is a kind of light brown, resembling the colour of a certain sort of Spanish snuff. The 170 feet object-glass showed the same colour also very clearly.

A light brown seen.

XIII.

XIII. *Of the order of the Colours.*

The arrangement of the colours in each compound ring or alternation, seen by reflection, is, that the most refrangible rays are nearest the centre; and the same order takes place when seen by transmission. We have already shown, that, when a full dilution of the colours was obtained, their arrangement was violet, indigo, blue, green, yellow, orange, and red; and the same order will hold good, when the colours are gradually concentrated again; for though some of them should vanish before others, those that remain will always be found to agree with the same arrangement. The most refrangible rays nearest the centre

If the rings should chance to be red and green alternately, a doubt might arise, which of them is nearest the centre; but by the method of dilution, a little pressure, or some small increase of the focal length of the incumbent lens, there will be introduced an orange tint between them, which will immediately ascertain the order of the colours.

In the second set of rings the same order is still preserved as in the first; and the same arrangement takes place in the third set as well as in the fourth. In all of them the most refrangible rays produce the smallest rings. in all cases.

XIV. *Of the alternate Colour and Size of the Rings belonging to the primary and dependent Sets.*

When two sets of rings are seen at once, and the colour of the centre of the primary set is black, that of the secondary will be white; if the former is white, the latter will be black. The same alternation will take place if the colour of the centre of the primary set should be red or orange; for then the centre of the secondary one will be green; or if the former happens to be green, the latter will be red or orange. At the same time there will be a similar alternation in the size of the rings; for the white rings in one set will be of the diameter of the black in the other; or the orange rings of the former will be of equal magnitude with the green of the latter. Alternation of the dependent sets in colour and size.

When three sets of rings are to be seen, the second and third sets will be alike in colour and size, but alternate in both particulars with the primary set.

The

The same thing will happen when four sets are visible; for all the sets that are formed from the primary one will resemble each other, but will be alternate in the colour and dimensions of their rings with those of the primary set.

XV. *Of the sudden Change of the Size and Colour of the Rings in different Sets.*

The size and colour of the rings in different sets may be suddenly changed.

When two sets of rings are viewed, which are dependent upon each other, the colour of their centres, and of all the rings in each set, may be made to undergo a sudden change by the approach of the shadow of the point of a penknife or other opaque slender body. To view this phenomenon properly, let a 16-inch double convex lens be laid upon a piece of looking glass, and when the contact between them has been made to give the primary set with a black centre, that of the secondary will be white. To keep the lens in this contact, a pretty heavy plate of lead with a circular hole in it of nearly the diameter of the lens should be laid upon it. The margin of the hole must be tapering, that no obstruction may be made to either the incident or reflected light. When this is properly arranged, bring the third shadow of the penknife upon the primary set, which is that towards the light. The real colours of this and the secondary set will then be seen to the greatest advantage. When the third shadow is advanced till it covers the second set, the second shadow will at the same time fall upon the first set, and the colour of the centres, and of all the rings in both sets, will undergo a sudden transformation from black to white and white to black.

The alternation of the colour is accompanied with a change of size, for as the white rings before the change were of a different diameter from the black ones, these latter, having now assumed a black colour, will be of a different size from the former black ones.

When the weight is taken from the lens, the black contact will be changed into some other. In the present experiment it happened, that the primary set got an orange coloured centre, and the secondary a green one. The same way of proceeding with the direction of the shadow being then pursued, the orange centre was instantly changed to a green

green one, while at the same moment the green centre was turned into orange. With a different contact I have had the primary set with a blue centre and the secondary with a deep yellow one; and by bringing the second and third shadows alternately over the primary set, the blue centre was changed to a yellow, and the yellow centre to a blue one; and all the rings of both sets had their share in the transformation of colour and size.

If there are three sets of rings, and the primary set has a black centre, the other two will have a white one; and when the lowest shadow is made to fall on the third set, the central colour of all the three sets will be suddenly changed, the first from black to white, the other two from white to black.

A full explanation of these changes, which at first sight have the appearance of a magical delusion, will be found in a future article.

XVI. *Of the Course of the Rays by which different Sets of Rings are seen.*

Determination of the course of the rays.

In order to determine the course of the rays, which give the rings both by reflection and by transmission, we should begin from the place whence the light proceeds that forms them. In Pl. IV, fig. 1, we have a plano-convex lens laid upon three slips of glass, under which a metalline mirror is placed. An incident ray 1, 2, is transmitted, through the first and and second surface of the lens, and comes to the point of contact at 3. Here the rings are formed, and are both reflected and transmitted: they are reflected from the upper surface of the first slip, and pass from 3 to the eye at 4: they are also transmitted through the first slip of glass from 3 to 5; and at 5 they are again both reflected and transmitted; reflected from 5 to 6, and transmitted from 5 to 7; from 7 they are reflected to 8, and transmitted to 9; and lastly they are reflected from 9 to 10. And thus four complete sets of rings will be seen at 4, 6, 8, and 10.

The most convenient way of viewing the same rings by transmission is that, which has been mentioned in the second article of this paper, when light is conveyed upwards by reflection. In figure 2, consisting of the same arrangement of glasses as before, the light by which the rings are to be seen

seen comes either from 1, 2, or 3, or from all these places together, and being reflected at 4, 5, and 6, rises up by transmission to the point of contact at 7, where the rings are formed. Here they are both transmitted up to the eye at 8, and reflected down to 9; from 9 they are reflected up to 10 and transmitted down to 11; from 11 they are reflected to 12 and transmitted to 13; and lastly, from 13 they are reflected to 14; so, that again four sets of rings will be seen at 8, 10, 12, and 14.

This being a theoretical way of conceiving how the rays of light may produce the effects, it will be required to show by experiments, that this is the actual progress of the rays, and that all the sets of rings we perceive are really reflected or transmitted in the manner that has been pointed out; but as we have so many reflections and transmissions before us, it will be necessary to confine these expressions to one particular signification when they are applied to a set of rings.

What is meant by reflected

When the centre of the rings is seen at the point of contact, it is a primary set; and I call it reflected, when the rays which come to that point and form the rings undergo an immediate reflection. But I call it transmitted, when the rays, after having formed the rings about the point of contact, are immediately transmitted.

and transmitted.

Thus in figure 3 and 4 the rays *a b c*, *d e f*, give reflected sets of rings; and the rays *g h i*, *k l m*, in figure 5 and 6, give transmitted sets.

In this denomination, no account is taken of the course of the rays before they come to *a*, *d*, *g*, *k*; nor of what becomes of them after their arrival at *c*, *f*, *i*, *m*: they may either come to those places or go from them by one or more transmissions or reflections, as the case may require; but our denomination will relate only to their course immediately after the formation of the rings between the glasses.

The secondary and other dependent sets will also be called reflected or transmitted by the same definition: and as a set of these rings formed originally by reflection may come to the eye by one or more subsequent transmissions; or being formed by transmission, may at least be seen by a reflection from some interposed surface, these subsequent transmissions

or

or reflections are to be regarded only as convenient ways to get a good sight of them.

With this definition in view, and with the assistance of a principle which has already been proved by experiments, we may explain some very intricate phenomena; and the satisfactory manner of accounting for them will establish the truth of the theory relating to the course of rays that has been described.

The principal to which I refer is, that, when the pressure is such as to give a black centre to a set of rings seen by reflection, the centre of the same set, with the same pressure of the glasses, seen by transmission will be white*.

I have only mentioned black and white, but any other alternate colours, which the rings or centres of the two sets may assume, are included in the same predicament.

XVII. *Why two connected Sets of Rings are of alternate Colours.*

Why two connected sets are of alternate colours.

It has already been shown, when two sets of rings are seen, that their colours are alternate, and that the approach of the shadow of a penknife will cause a sudden change of them to take place. I shall now prove, that this is a very obvious consequence of the course of rays that has been proposed. Let figure 7 and 8 represent the arrangement given in a preceding article, where a 16-inch lens was laid upon a looking glass, and gave two sets of rings with centres of different colours: but let figure 7 give them by one set of rays, and figure 8 by another. Then, if the incident rays come in the direction which is represented in figure 7, it is evident that we see the primary set with its centre at 2 by reflection, and the secondary one at 4 by transmission. Hence it follows, in consequence of the admitted principle, that if the contact is such as to give us the primary set with a black centre, the secondary set must have a white one; and thus the reason of the alternation is explained.

But if the rays come as represented in figure 8, we see the primary set by transmission, and the secondary one by reflection; therefore, with an equal pressure of the glasses, the

* See Article XI, of this Paper, p. 135.

primary

primary centre must now be white, and the secondary one black.

Without being well acquainted with this double course of rays, we shall be liable to frequent mistakes in our estimation of the colour of the centres of two sets of rings; for by a certain position of the light, or of the eye, we may see one set by one light, and the other set by the other.

XVIII. *Of the Cause of the sudden Change of Colours.*

Cause of the sudden change of colour.

Having thus accounted for the alternation of the central colours, we may easily conceive, that the interposition of the penknife must have an instantaneous effect upon them. When it stops the rays of figure 7, which will happen when its second shadow falls upon the primary set, the rings will then be seen by the rays 1, 2, 3, 4, and 1, 2, 3, 5, 6, of figure 8. When it stops the rays of figure 8, which must happen when the third shadow falls upon the primary set, we then see both sets by the rays 1, 2, 3, and 1, 2, 4, 5, of figure 7. When the penknife is quite removed, both sets of rays will come to the point of contact, and in some respects interfere with each other; but the strongest of the two, which is generally the direct light of figure 7, will prevail. This affords a complete explanation of all the observed phenomena: by the rays of figure 7 the centres will be black and white; by those of figure 8 they will be white and black; and by both we shall not see the first set so well as when the third shadow, being upon it, has taken away the rays of figure 8: indeed we can hardly see the secondary set at all, till the shadow of the penknife has covered either the rays of figure 7 or of figure 8.

Both the centres and rings may be changed partially.

As soon as we are a little practised in the management of the rays, by knowing their course, we may change the colour so gradually as to have half the centre white, while the other half shall still remain black: and the same may be done with green and orange, or blue and yellow centres. The rings of both sets will also participate in the gradual change; and thus what has been said of the course of rays in the 16th article will again be confirmed.

To be concluded in our next.

VIII.

Nicholson's Philos. Journal, Vol. XIX. Pl. IV, p.139.

Dr. Herschel on coloured concentric Rings.

Fig. 1.

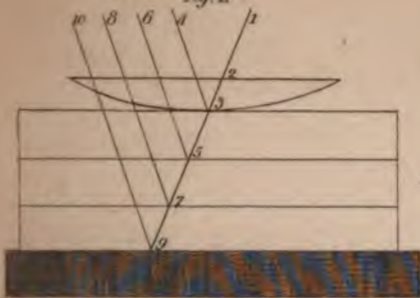


Fig. 2.

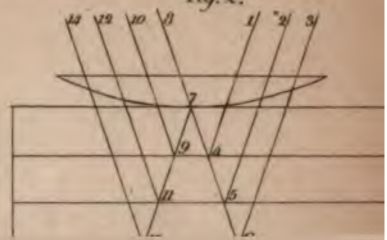


Fig. 3.

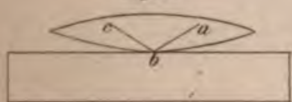


Fig. 4.

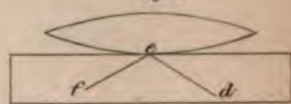


Fig. 5.

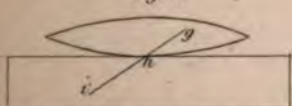


Fig. 6.

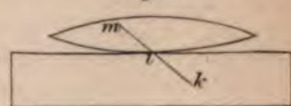


Fig. 7.

Fig. 8.

VIII.

A Method of finding the specific Gravity of Light from Analogy; and the undulatory System defended by an Experiment on inflected Light. In a Letter from Mr. RICHARD WINTER.

To Mr. NICHOLSON.

DEAR SIR, *Whitby, Jan. 8th,* 1808.

THE undulatory system of light had until within very lately become exploded by the extraordinary abilities of Newton, and his great exertions in favour of the emanative system; but no name, however great, should prevent inquiry after truth and extension of science, so naturally allied to the civilization and happiness of mankind. It is, I believe, generally allowed, that few discoveries have been made by pursuing a beaten path; it is on this account that so few improvements have been made in the theory of light since the time of Sir Isaac Newton. Dr. Young's experiments, and reasoning from facts, in favour of the undulatory motion of light, are deserving of impartial attention. Undulatory system of light beginning again to claim attention.

The great influence of light on vegetables and animals is ascertained, from the want of colour in both when deprived thereof; and the vigour, odour, and density of tropical plants, and the ferocity of animals indigenous to those climates. Its consequences in the arts and manufactures are very considerable, in its various combinations with the elementary bodies. Its effect also upon man is acknowledged and felt by all nations, so as to contribute a principal characteristic (viz. that of colour). Light has great influence on vegetables and animals; in arts and manufactures; on man;

The physical phenomena arising therefrom display a wide field for the investigation of the natural philosopher, in the production and change of colours—the formation of the rainbow, parhelia, haloes, &c. and on natural phenomena.

It has baffled the ingenuity of man to determine its density by mechanical means. Michell attempted to find its momentum Its density not easy to ascer-

tain mechanically.

momentum upon a balance, but the transmission through different glasses will vary, as the lenses may happen either to differ in density or transparency; and consequently will give different results. You also advanced (Introduction to Natural Philosophy) some ingenious arguments to decide its amazing subtlety, founded upon undoubted principles.

May perhaps be deduced analogically.

The following analogy will appear perhaps hypothetical; however, such as it is I will submit it to the candid and discriminating, to determine whether the conclusions are substantial or premature.

Undulations of mediums as their gravities.

The resistance of fluids is as their densities reciprocally; therefore it may be presumed, that the undulations of different mediums will bear the same proportion to one another, as their specific gravities.

Velocity of those of light.

It has been demonstrated by the accurate observations and discoveries of Dr. Bradley on the aberriation of light, that this medium is conveyed from the sun to the earth, or in other words, an undulation of light reaches the earth from the sun, in the space of 8′ 7·5″ of time. Taking the mean apparent diameter of the sun at 32′ 1·5″, and his mean horizontal parallax at 8·72″, as determined by Dr. Maskelyne, and the semidiameter of the earth at 3964 miles, we shall find the sun's real diameter to be 873,489 miles, and his distance from the earth equal to 93,334,047 miles: therefore the velocity of light will be determined thus, $\frac{93334047}{8'\ 7\cdot5''} =$ 191,434 miles in one second of time, or 1,010,771,520 feet.

Undulations of air and water compared.

According to Hales (Statics, Vol. II, p. 331) the velocity of undulating air is to the velocity of undulating water as 865 to 1, or as their specific gravities. The motion of sound is found to be 1130 feet in one second (Young's Syllabus of a Course of Lectures, 1802): then, as the velocity of sound is to the velocity of light, so is the specific gravity of air to the specific gravity of light, according to the following formula; $\frac{1010771520}{1130} = 894{,}588$ times lighter than atmospheric air; or it will require 1553 cubic feet of light to weigh one grain. If we compare them with water, taken as unity

Hence light 894588 times lighter than air.

unity, we shall find them expressed as follows at a mean temperature.

SPECIFIC GRAVITIES.

Water	1·00000000000
Air	0·00120000000
Light	0·00000000013

If this be the real density of light, it will appear, that all former attempts to appreciate its specific gravity by mechanical means must have been fruitless, as the quantity thrown by a lens, however large, upon a balance of the most delicate construction, must be exceedingly minute; yet it may have very considerable effects when exerted upon the body of the planets. May not the diurnal motion of the planets be the effect of its momentum?

Hence incapable of being weighed directly.

It appears to me, that the experiment on inflected light, mentioned in Newton's optics, performed by passing the light through an aperture of a window shutter into a darkened room, is much better explained, by allowing an undulatory than a radiating motion of light.

Inflection of light better explained by undulation than radiation.

It is the nature of all fluids to undulate in circular arcs when moved by any impulse.

Let *a* represent an aperture into a darkened room, equal to $\frac{1}{12}$ part of an inch in diameter, *b*, *c*, *d*, *e*, *f*, waves of light, moving in succession against the solid object *g h*, which we will suppose the side of a house: here the light, meeting with an opake substance, will be reflected every where, except at the aperture *a*, which will then become the centre of motion. The undulating light, having passed the aperture, will dilate in the concentric arcs 1, 2, 3, 4, &c., till they arrive at *i*, on the opposite side of the room; and the greater the distance between *a* and *i*, the greater will be the diameter of the shadow of the aperture; all obstacles placed in this lucid stream will have their shadows augmented in diameter, when received upon the wall, in proportion to their distance therefrom.

Phenomena of light admitted through a small aperture.

If the attraction of the sides of the aperture and window shutter was the cause of this enlargement of the shadow, the obstacle, when interposed in the lucid stream within the room, would also attract the light, and the diameter of its shadow,

Not owing to its being attracted by the sides of the aperture.

shadow, instead of being augmented (as it really appears to be), would be diminished.

I am very respectfully,

Your obedient servant,

RICHARD WINTER.

It gives me great pleasure to observe, that you have undertaken to publish an Encyclopedia upon a limited scale. It will be peculiarly adapted to the interest of the artizan, the mechanic, the manufacturer, and to the most numerous class of society.

There is one article which would be useful to your country readers, I mean a Monthly Meteorological Register inserted in the Journal, of the Barometer, Thermometer, Winds, &c. at London, in order to enable them to compare them with observations made in the country; perhaps this may be inconsistent with your plan, which is generally approved.

As it is my wish to gratify all my readers, in whatever tends to promote the interests of science, I shall take measures to comply with the request of my correspondent, by inserting, as soon as conveniently can be done, a meteorological register, from a hand that may be relied on with confidence for its accuracy. W. N.

IX.

Account of an Accident from the sudden Deflagration of the Base of Potash. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Caution against accidents in decomposing the alkalis.

AS the late brilliant discoveries by Mr. Davy, of the decomposition of the fixed alkalis, will probably induce many to repeat his experiments, I take the liberty of suggesting to them, through the medium of your Journal, the *caution of using glasses* to defend the eyes during the operation. The flat glasses, commonly called *goggles*, are best adapted to the purpose.

For want of this precaution, I yesterday met with an accident, from which I have suffered much pain, and might even have been totally deprived of sight by it. A considerable

able quantity of potash being decomposed in the galvanic circle, a sudden deflagration of the metallary base ensued, by which several particles of the caustic alkali were thrown into my eyes.

Potash being decomposed, its base suddenly deflagrated, and several particles thrown into the operator's eyes.

To prevent the like accident happening to others, who may be engaged in similar experiments, is my motive for sending you this. Whether it is worth your notice or not, you will judge.

I remain, SIR,

Your obedient servant,

Tunbridge, Jan. 22, 1808. PHILOMMATOS.

P. S. I lose no time in making the communication, but my eyes are still so weak, I can scarcely see to write.

X.

Correction of some Misstatements in the Account of Mr. Davy's Decomposition of the fixed Alkalis. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR, *London, Jan.* 24, 1808.

THE extensive circulation of your excellent Journal both at home and abroad makes it more desirable, that it should not be the means of propagating any incorrect statements of scientific facts; and such statements are given in the account of Mr. Davy's important discovery of the decomposition of the fixed alkalis.

Misstatements in the account of the decomposition of the fixed alkalis.

I was present at the reading of his lecture. I paid the greatest attention to it. I feel that your well known love of philosophical justice will induce you, to give a place in your publication to what I am convinced were the real accounts of the author.

It is stated in your Journal, that the basis of potash is volatile at 100°. Mr. Davy's account was, that it is volatile at a heat a little below redness. It is likewise said, that the amalgam of the basis of potash and quicksilver, when applied in the circle of a galvanic battery, dissolved iron, silver, gold, and platina. Mr. Davy merely mentioned, that it dissolved these metals; he said nothing, that I can recollect, of the galvanic battery.

Basis of potash volatile a little below a red heat.

Its amalgam dissolves metals.

It decomposes glass by combining with its alkali into an oxide with less oxigen than potash.

Glass, it is said in your Journal, is dissolved by the basis of potash in the same manner as the metals. The real statement with regard to glass was, that the basis of potash decomposed it by combining with its alkali, and by forming a red oxide of a less degree of oxigenation than potash, which oxide was likewise procured by other means.

Spec. grav. of the base of soda 0·9.

It is stated, that the specific gravity of the basis of soda is to that of water as 7 to 10. Mr. Davy said, as 9 to 10.

I am, Sir, with great respect,
Your obedient humble servant,
A CHEMIST.

XI.

An Improvement in the Galvanic Trough, to prevent the Cement from being melted, when the Action is very powerful. Communicated by a Correspondent.

SIR, To Mr. NICHOLSON.

Cement of the galvanic trough liable to be melted by the heat evolved.

THE superiority of galvanic batteries constructed on the principle of Volta's *couronne des tasses*, as recommended by Mr. Wilkinson, is, I believe, fully established. One inconvenience however attends it: the action of the acid on the zinc plates being greatly increased, the quantity of caloric evolved is so considerable, as frequently to melt the cement with which the *wooden* partitions of the troughs are covered. To remedy this inconvenience, I have had recourse to glass partitions, and find them answer my expectations completely. It is better to make them so much larger than the metallic plates, as to leave a space of about half an inch (it should not I think be less) between the sides and bottom of the trough, and metallic plates. Common crown glass is perfectly adapted to the purpose: its thickness, of course, must be proportioned to the size required; and the *top edge* should be ground smooth. A battery constructed on this plan may be excited to great intensity, without injuring the cement at all.

This may be remedied by making the partitions of glass.

This battery may be excited to great intensity.

I have the honour to be, Sir,
Your obedient servant, J. G. C.

Tenbridge, Jan. 24, 1808.

XII

XII.

Experiments on the Fire-damp of Coal Mines, by William Henry, *M. D.; including a Communication on the Subject from* Thomas Thomson, *M. D. F. R. S. E. Communicated by Dr. Henry.*

History of the fire-damp subjected to experiment.

ABOUT the close of 1806, I received, from the Rev. William Turner of Newcastle on Tyne, two bladders filled with the fire-damp, which had been procured from a coal mine in the neighbourhood of that town. It was caught by luting a common funnel over the mouth of a *blower* *, and tying a compressed bladder to the pipe of the funnel, after the gas had issued from it for some time. My experiments were made on the gas, about seven days after its being first collected. At that time, the bladders were perfectly dry, and showed no signs of putrefaction.

General results.

The general results of these experiments (as stated in a memoir which was read in January 1807, before the Medical Society of Edinburgh) are the following. The gas was found, by the test of nitric oxide, used in Mr. Dalton's method †, to contain about $\frac{1}{5}$ its bulk of common air. It had a disagreeable smell. When set on fire as it issued from the orifice of a small pipe, it burned with a dark blue flame; and a long conical glass vessel, held over the flame, was soon bedewed with moisture. Mixed with common air, it did not detonate on the approach of a lighted taper, at least in any proportion that was tried. The utmost effect was a deep blue flame, which spread quickly through the vessel, but was not accompanied with any noise. With oxigen gas, however, it exploded, and gave a loud report. On agitation with limewater it lost about $\frac{1}{50}$ of its bulk. The nicest tests did not discover any admixture of sulphuretted hidrogen. One hundred parts by measure appeared, therefore, to consist of

* *Blowers* are holes or crevices in the coal or in the accompanying strata, from which the fire-damp issues, sometimes with considerable force.

† Phil. Journ. XVI, 247; or Henry's Epitome, chap xii, sect. 2.

63·33

Component parts.

63·34	atmospherical air
1·66	carbonic acid
35·	inflammable gas
100·0	

The inflam. gas was carburetted hidrogen.

The nature of the inflammable gas was next ascertained by detonation with oxigen gas. Reducing the results to a general average, and excluding the common air, the really inflammable part of the gas required for combustion about twice its bulk of oxigen; and gave its own volume of carbonic acid. Hence the inflammable portion of the gas was *carburetted hidrogen*. From the experiments of Mr. Dalton on the gas from stagnant water, and my own obtained by distilling pit-coal*, the fire-damp appears to differ very little from both those gasses.

Fire-damp less adulterated was examined by Dr. Thomson.

It was desirable, however, to repeat the analysis of fire-damp, less adulterated with common air; and for this purpose a quantity was collected (as it issued through water on the floor of the mine) in an inverted bottle, which was well corked and tied over with bladder. Happening to pass through Newcastle last spring, I carried this gas with me to Edinburgh; and, having no opportunity of making experiments upon it there, my friend Dr. Thomson was so good as to undertake its analysis, and to furnish me with the following results.

Detail of the experiments and result.

From the action of nitrous gas and of lime-water, the gas appeared by Dr. Thomson's experiments, to contain, in 100 measures,

63	inflammable gas
6·5	oxigen
25·5	azote
5·	carbonic acid
100·0	

* The gas obtained by the destructive distillation of coal I have found to contain a variable proportion of sulphuretted hidrogen, and to differ somewhat from the composition which I have stated in the 11th vol. of this Journal. The correction of those results I reserve for another occasion.

The

The following TABLE shows the result of its combustion, performed by detonating it over water. The oxigen gas contained $77\frac{1}{2}$ pure oxigen, and $22\frac{1}{2}$ azotic gas per cent.

Experiment.	Measures of Fire-damp.	Measures of Oxigen.	Residue after combustion.	Ditto after Lime-water.	Nitrous Gas add. to residue.	Residue of Nit. Gas & Fire-d.
1	20	30	27	22	17	38
2	20	35	24	14	13	25·5
3	20	40	27	15·5	20	25
4	10	30	24	18	30	27
5	10	50	43	36	45	21
6	10	40	33	27	40	28·5

The results of the foregoing experiments are explained as follows.

In Experiment.	Real infl. Gas used.	Pure oxigen used.	Azote.	Residue.	COMPOSITION OF THE RESIDUE. Carbonic acid Gas.	Azote.	Oxigen.	Infl. Gas unconsumed.
1	12·6	24·55	11·85	27	5	11·85	0·37	9·78
2	12·6	28·43	12·97	24	10	12·97	0·55	0·48
3	12·6	32·30	14·10	27	11·5	14·10	0·40	0·
4	6·3	23·90	9·30	24	6	9·30	7·70	1·
5	6·3	39·40	13·80	43	7	13·80	22·	0·20
6	6·3	31·65	11·55	33	6	11·55	14·17	1·28

It

It appears, therefore, that, when the gas was entirely consumed, 12·6 measures of the really inflammable part gave 11·5 measures of carbonic acid, and required for saturation about 32 measures of oxigen. The average results are the following, excluding the first experiment in which the combustion was far from being perfect.

	Over water.	Over mercury.
Measures of oxigen required for saturating 100 measures of fire-damp	269·4	246·
Measures of carbonic acid produced	98·4	90·6

The second column contains the average results of two experiments, which Dr. Thomson made over mercury; but on these he places less reliance than on the foregoing series. The general issue of his experiments agrees with that of mine; and the difference is chiefly in the quantity of oxigen consumed by the combustion of the fire-damp, which appeared to me not to exceed twice its volume.

The fire-damp is not produced from decomposed pyrites, nor from water decomposed by coal; but probably from coal distilled by the heat of pyrites.

I know not whether the result of the foregoing experiments will be considered as affording any insight into the nature of the process, by which the fire-damp is generated in coal mines. The entire absence of sulphuretted hidrogen gas shows, that it is not the immediate product of the decomposition of water by beds of pyrites; for in that case, the evolved hidrogen would undoubtedly have dissolved a portion of sulphur. Neither can it arise from the decomposition of water by coal; for, besides that coal has no action on water at a moderate temperature, this origin is contradicted by the smallness of the proportion of carbonic acid which is present in the fire-damp. The most probable supposition is, perhaps, that it is disengaged from coal by a kind of natural distillation. The heat required for this purpose may be communicated by contiguous beds of pyrites; and may be excited in them by the occasional influx of water. In confirmation of this opinion it may be observed, that the fire-damp is generated most abundantly after long and heavy rains. The freedom, also, of some coal mines from this destructive gas, indicates the operation of a partial cause.

It

It is to be regretted, that the analysis of the fire-damp affords no encouragement to expect, that it can ever be destroyed in coal mines by any chemical process, as has lately been proposed. The only feasible method of preventing the dreadful consequences of its combustion is, to enforce the steady execution of a well planned system of ventilation, not only in the part of the mine actually in work, but in the old workings or *waste*. Every accident which has happened may, I have been informed, be traced either to an errour in the method of ventilation, or to neglect of its enforcement. The most important object, therefore, appears to be, the improvement of the mode of ventilating coal mines; and especially the superseding, by proper mechanical contrivances, the necessity of those attentions which are at present required on the part of the workmen. The peculiar expediency of changing the air of a mine, after an accidental explosion, before venturing into it, is apparent from the foregoing experiments, which show, that, after every such combustion, a large quantity of that gas must have been generated, which is known to miners under the name of *choak damp*.

Hence the only method of opposing the destructive effects of fire-damp is to ventilate well.

Manchester, Jan. 10, 1808.

XIII.

On the Phosphorescence of Bodies, from the Action of the Electric Explosion. In a Letter from Mr. WILLIAM SKRIMSHIRE, *Jun. to Mr.* JOHN CUTHBERTSON.

DEAR SIR, *Wisbech, Jan.* 5, 1808.

I Have lately resumed my Electrical Experiments, and having gone through the inflammables, as also the metals, metallic ores, and oxides, I take the liberty of sending them to you. Respecting the phosphoric appearance of bodies, these few experiments are no otherwise interesting, than as forming additional links in the chain of facts, which I have formerly stated, and which it is my intention to extend throughout the animal and vegetable kingdoms. But

Continuation of electrical experiments.

as

as they discover to us a whole class of bodies devoid of the least phosphorescency, after exposure to electric light, they may be deemed of some importance, especially as being one step towards leading us to a theory of this phenomenon. I trust you will be kind enough to send them to Mr. Nicholson, to be inserted in his extremely useful Journal.

Combustibles.

Habitudes of bodies as to the electric spark and the production of phosphorescence.

Sulphur.

Sulphur. 1st, Roll brimstone gives no spark, and is scarcely at all luminous by the shock. 2d, Flowers of sulphur are not phosphoric. 3d, A native specimen pure gave no spark, and was very slightly luminous by the shock. 4th, A native specimen mixed with carbonate of lime gave no spark, but was more luminous than the preceding specimens.

Phosphorus.

Phosphorus inflames both by the spark and shock.

Charcoal.

Charcoal. Some kinds afford very good sparks, are phosphorescent upon the surface, and when the rods rest upon them the dust is dispersed by the explosion, in the forms of a luminous cloud. But other pieces, which were tried, did not become phosphoric by exposure to the electric light.

Coke.

Coke gives a good spark, but is not luminous by the shock.

Cannel coal.

Cannel coal and common Sunderland coal give sparks beautifully variegated in minute spangles radiated upon its surface, but they are not phosphoric. Welsh coal gives similar sparks, but not so beautiful as the above; and is not luminous by the shock.

Peat.

Peat, hard and dry, affords a very good spark, but is scarcely luminous.

Soft, porous, very light peat, termed in this neighbourhood Ramsay turf, is not luminous except in the track of the discharge, and even then it is extremely evanescent.

Charred peat affords a very good spark, and is slightly luminous.

Bitumen.

Bitumen, hard and brittle, of a dark brown colour, from Derbyshire, gives no sparks, but the fluid spreads uniformly and silently over its whole surface, to pass from the conductor to the knob of the discharger held above it, with an appearance similar to the electric light in an exhausted receiver. It is luminous by the shock, as is also the elastic bitumen from the same country.

Jet

Jet and asphaltum, instead of a spark, afford the same appearance of electric light as bitumen does; but they are not luminous by the explosion. Jet. Asphaltum.

Amber gives no spark, but is phosphorescent, especially that kind termed fat amber. Amber.

Plumbago gives good sparks, and is not phosphoric; but when mixed with clay, and manufactured into crucibles, it affords good sparks, which are flame-coloured and purple upon the surface, and becomes luminous when the shock is taken *above* its surface. Plumbago.

Metals, their Ores and Oxides.

As the metals are excellent conductors of electricity, it is well known that they all afford good sparks; but I have not been able to perceive any material difference in the colour of the electric light, from different metals, unless the metal has been formed into exceeding thin leaves, or otherwise minutely divided, and the spark be sufficiently strong to produce oxidation.

Not one of the metals is phosphoric by exposure to the light of an electric explosion, if its surface be clean and bright. The metals are not phosphoric.

This is the only class of natural bodies, which I have yet found uniformly to remain dark after exposure to the electric light. Some of their ores and oxides, such as the red and yellow arsenic, hæmatites, pyrites which is found in chalk, oxide of zinc, and oxide of antimony are very slightly luminous; whilst others, for instance cinnabar, black sulphuret of mercury or ethiops mineral of the shops, mundic, galena, blend, and the sulphurets of antimony, minium, litharge, and some other oxides, as readily absorb, and as obstinately retain within their substance, the electric light to which they are exposed, as even the metals themselves. In short I have not met with a single brilliant phosphoric appearance in any of the metals, ores, or oxides, which I have had the opportunity of subjecting to experiment. These observations entirely coincide with the results of experiments on solar phosphori by Beccari, who tried every means that his inventive genius could suggest, to render the metals phosphoric, but without success. However it is not surprising, that there should be a tolerably exact agreement between the observa- These experiments agree with those of Beccari on solar phosphori.

tions

tions of Beccari on solar phosphori, and those facts, which are stated in my letters. For as I think there can be no doubt but the phosphoric appearance of bodies in the dark, after exposure to the light of the sun, and the phosphorescency of those substances that have been exposed to the light of an electric explosion, proceed from the same cause, they must necessarily be subject to similar laws.

But this subject will claim our attention more particularly hereafter, when I shall have occasion to speak of the nature and cause of this phosphoric phenomenon.

I remain, Your's, &c.

WILLIAM SKRIMSHIRE, Jun.

XIV.

Experiments on the Decomposition of the fixed Alkalis by Galvanism. In a Letter from Mr. Charles Sylvester.

To Mr. NICHOLSON.

Dear Sir,

Farther experiments on the decomposition of alkalis.

BEING on a visit, for a few days, with my friend, Mr. Oakes, Jun. of Derby, we have together made some experiments, in prosecution of the inquiry instituted by Mr. Davy, relative to the decomposition of the fixed alkalis by the galvanic influence; the result of whose research has been recently communicated to the Royal Society.

Potash exposed to the action of a surface of 1400 inches.

In the first experiment, we used a pair of troughs, exposing a surface of 1400 square inches, and placed the potash, which was perfectly pure and white, on a plate of platina; but did not moisten it, as is said to have been the case in Mr. Davy's experiments, the deliquescence of the alkali precluding the necessity of such precaution. As soon as the platina wire was brought into contact with the potash, from the opposite

Gas evolved.

end of the battery, a considerable quantity of gas was evolved; arising most probably from a decomposition of the

The alkali blackened and emitted sparks.

water. The alkali, in consequence, assumed a blackish colour, which continued to be produced so long as the action was maintained, sparks being frequently emitted; which latter effect has only been observed to take place with charcoal and the metals.

Exposed to the

A second experiment was made, with the addition of another

ther pair of troughs of the same size as the first; and to remedy the inconvenience occasioned by the deliquescence of the potash in the former attempt, a glass tube was employed, having a platina wire, coiled into a spiral form, sealed into one of its ends. The alkali was placed in the tube, surrounded by the spiral wire, and another wire, passing through a cock which occupied the other end of the tube, was, by sliding freely up and down, made to touch the potash at intervals. The wires being connected with the battery, and the alkali slightly moistened, a considerable portion of gas was evolved, which from time to time exploded by the sparks produced: the temperature of the mass was materially increased, and the black matter, which was deposited on solution of the alkali in water, appeared in greater quantity than before. Small portions of this black substance sticking to the end of the wire, on being brought into contact with water, suddenly detonated accompanied with a vivid flash; an effect which was also produced on pouring distilled water into the tube.

action of 2800 inches, and deliquescence prevented by enclosing in a tube.

Appearances as before.

Temperature increased.

Black matter detonated on the contact of water.

The detonations caused by the black matter coming into contact with water, we ascertained from experiment, could not be produced by potash in any state of dryness; hence it would appear, that some substance has been created during the galvanic process, possessed of properties very different from those of the materials employed.

Potash does not produce this effect in any state.

Farther inquiry promised.

It is our intention however, to resume these experiments assisted by greater galvanic power, the result of which I shall transmit to you.

I am, Sir, Your obedient servant,

Derby, 20th *Jan.* 1808. CHA. SYLVESTER.

SCIENTIFIC NEWS.

Discovery of a complete Mammoth.

THE bones that have been discovered in different parts of the northern hemisphere sufficiently prove the existence of some large animal, or animals, now unknown; and some writers have even given a particular description of the quadruped generally called a mammoth, though it would seem merely from the report of tradition among the uncultivated

Mammoth found in a perfect state.

tivated nations of the north. Lately however one has been found, not alive indeed, but complete, and in a state of perfect preservation, on the borders of the Frozen ocean. The following is the account, that has been received of it from Petersburg.

Account of its discovery.

Schoumachoff, a Tungoose chief, about the end of august 1799, when the fishing in the river Lena was over, repaired according to annual custom to the seaside. Leaving his family in their huts, he coasted along the shore in quest of mammoth's tusks, and one day perceived in the midst of a rock of ice a large shapeless block, not at all resembling the logs of drift wood commonly found there. He climbed the rock, and examined it all round, but could not make out what it was. The next year, visiting the same spot, he found there the carcase of a seacow *(trichecus rosmarus)*; and observed, not only that the mass he had seen the year before was freer from ice, but that there were two similar pieces by the side of it. These afterward turned out to be the feet of the mammoth. In 1801, the side of the animal and one of its tusks appearing very distinctly, he acquainted his wife and some of his friends with what he had found. This however gave them great alarm, for the old men said, that they had been told by their forefathers a similar monster was once before seen in those parts, and the whole family of the person who discovered it soon became extinct. At this Schoumachoff was so much alarmed, that he fell sick. On his recovery however he could not relinquish the expectation of the profit he might make of the tusks; and directed his servants to conceal the circumstance carefully, and endeavour to keep away all strangers by some pretext or other. It was not till the fifth year, that the ice had melted sufficiently to disengage the mammoth, when it fell over on its side upon a bank of sand. Schoumachoff then cut off the tusks, which he bartered for goods to the value of 50 rubles [£11. 5*s*.] with a Russian merchant. Being satisfied with this, the carcase was left to be devoured by the bears, wolves, and foxes, except what the Yakouts in the neighbourhood cut off to feed their dogs. Previous to this indeed he had a rude drawing made of it, which represents it with pointed ears, very small eyes, horse's hoofs, and a bristly mane

Tradition of another.

Its flesh eaten by dogs and wild beasts.

Drawing of it.

mane extending along the whole of its back. In this it has the appearance of something between a pig and an elephant.

In 1806, Mr. Mich. Adams, of Petersburg, being at Yakoutsk, fortunately heard of this circumstance, and repaired to the spot. When he arrived there, the skeleton, nearly stripped of its flesh, was entire, one of the forefeet excepted. The vertebræ, from the head to the os coccygis, one of the shoulderblades, the pelvis, and the remaining three extremities, were still held firmly together by the ligaments of the joints, and by strips of skin and flesh. The head was covered with a dry skin. One of the ears, well preserved, was furnished with a tuft of bristles. These parts could not avoid receiving some injury during their removal to Petersburg, a distance of 11000 wersts [6875 miles]: the eyes however are preserved, and the pupil of the left eye is still distinguishable. The tip of the under lip was eaten away; and the upper being destroyed, the teeth were exposed. The brain, which was still within the cranium, appeared dry. The parts least damaged were one of the forefeet and one of the hind: these were still covered with skin, and had the sole attached to them. Description of it.

According to the Tungoose chief the animal was so corpulent and well fed, that its belly hung down below the knee joints. It was a male, with a long mane, but had neither tail nor trunk. From the structure of the os coccygis however, Mr. Adams is persuaded, that it had a short thick tail: and from the smallness of its snout, and the size of its tusks, he conceives it could not have been able to feed without the assistance of a proboscis; but Schoumachoff persisted in the assertion, that he never saw any appearance of a trunk, and it does not appear probable, that even his rude draughtsman would have omitted such a striking feature. The skin, three fourths of which are in possession of Mr. Adams, the part that lay on the ground having been preserved, was of a deep gray colour, and covered with reddish hair and black bristles. These, from the dampness of the ground, had lost some part of their elasticity. More than a poud [40 lbs.] weight of them, that had been trodden into the ground by the bears, was collected, many of them an archine [2 feet 4 in,] long. What remained of the skin was so heavy, that ten persons

persons found great difficulty in carrying it to the seaside, in order to stretch it on logs of wood. The head weighs $11\frac{1}{2}$ pouds [460 lbs.]; the two horns, each of which is $1\frac{1}{2}$ toise [$9\frac{1}{2}$ feet] long, weigh 10 pouds [400 lbs.]; and the entire animal measured $4\frac{1}{2}$ archines [$10\frac{1}{2}$ feet] high, by 7 [$16\frac{1}{2}$ feet] long. Mr. Adams has seen tusks of the mammoth so curved as to form three fourths of a circle; and one at Yakoutsk $2\frac{1}{2}$ toises [15 feet 9 in.] long, an archine [2 feet 4 in.] thick near the root, and weighing 7 pouds [280 lbs.]. They are curved in the direction opposite to those of the elephant, bending toward the body of the animal; and the point is always more or less worn on the outside, so that the right tusk is easily distinguishable from the left. He adds, that he found a great quantity of amber on the shores.

Its dimensions.

Amber.

We understand he wishes to dispose of the skeleton, and means to employ the money in a journey toward the north pole, and particularly in visiting what is called the island of Ljachow, or Sichow, which, from the information he has received, he believes to be part of the continent of North America.

St. Thomas's and Guy's Hospitals.

The Spring Course of Lectures at these adjoining Hospitals, will commence the beginning of February: viz. at

St. THOMAS'S,

Anatomy and the Operations of Surgery, by Mr. Cline, and Mr. Cooper.

Principles and Practice of Surgery, by Mr. Cooper.

GUY'S,

Practice of Medicine, by Dr. Babington and Dr. Curry.

Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen.

Experimental Philosophy, by Mr. Allen.

Theory of Medicine, and Materia Medica, by Dr. Curry and Dr. Cholmeley.

Midwifery, and Diseases of Women and Children, by Dr. Haighton.

Physiology, or Laws of the Animal Œconomy, by Dr. Haighton.

Structure and Diseases of the Teeth, by Mr. Fox.

N. B. These several Lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole are calculated to form a *Complete Course of Medical and Chirurgical Instructions.*—Terms and other Particulars may be learnt from Mr. Stocker, Apothecary to Guy's Hospital.

The communications from J. Gough, Esq., Dr. Gibbes, and N. R. D. will be given in our next.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH, 1808.

ARTICLE I.

Remarks on Torpidity in Animals, in two Letters from JOHN GOUGH, *Esq.*

SIR, *Middleshaw,* 16 *Jan.* 1808.

YOU have given, in your XVIIIth volume, page 254, an excellent memoir by Mr. du Pont de Nemours, on a kind of death, that may be presumed to be only apparent. This ingenious philosopher suggests several practical observations which merit the attention both of the benevolent and the curious, because they promise to promote the interests of humanity as well as of science. This writer, however, adopts one opinion, which perhaps is supported by the authority of antiquity, rather than facts and the known habits of animals. Mr. du Pont's memoir valuable.

Mr. du Pont agrees in opinion, perhaps with the majority of naturalists, respecting the nature of torpidity; for he refers it, partly to the benumbing effects of the cold which prevails in winter; and partly to a high degree of corpulence, which is generally contracted in autumn, from an unrestrained indulgence in the abundance and delicacies of that season. He moreover supposes, that animals do not submit to this long suspension of the vital functions in obedience to The prevailing explanation of torpidity stated.

the dictates of necessity; on the contrary, he imagines them to court a lethargic habit, in consequence of certain pleasing sensations, which are known to precede the first moments of sleep.

Objections to the preceding hypothesis.

The preceding hypothesis is commonly supposed to assign the true causes of torpidity; but the doctrine is liable to certain objections. I will state these in the first place, and afterwards endeavour to substantiate them by facts, which are new, or but imperfectly understood. My objections are contained in the four following propositions.

Objection 1st.

First, Animals do not submit to torpidity upon choice, but from necessity; and when cold happens to be the immediate cause, they fly from it, if possible.

Objection 2d.

Second, Certain animals apparently support a voluntary suspension of their functions in summer as well as winter, when food is withheld from them; this is probably intended to preserve life by diminishing the action of the system.

Objection 3d.

Third, A quadruped noted for its lethargic disposition in winter may be so far strengthened by a generous diet, as to retain the full use of its faculties during the time of a severe frost: from which we may infer, that an emaciated habit of body is the predisposing cause of torpidity, in opposition to the common opinion, which assigns this office to corpulence.

Objection 4th.

Fourth, The united action of hunger and a low temperature has produced a kind of apparent death in a human being, who was restored to life by stimulating remedies, after laying several days without sense and motion.

The first objection exemplified by the hearth cricket.

The hearth cricket *(gryllus domesticus)* affords a proof of the first objection. Those who have attended to the manners of this familiar insect will know, that it passes the hottest part of summer in sunny situations, concealed in the crevices of walls and heaps of rubbish. It quits its summer abode about the end of August, and fixes its residence by the fireside of the kitchen or cottage, where it multiplies its species, and is as merry at Christmas as other insects are in the Dog-days. Thus do the comforts of a warm hearth afford the cricket a safe refuge, not from death, but from temporary torpidity; which it can support for a long time, when deprived by accident of artificial warmth. I came to the

the knowledge of this fact, by planting a colony of these insects in a kitchen, where a constant fire is kept through the summer, but which is discontinued from November to June, with the exception of a day once in six or eight weeks. The crickets were brought from a distance, and let go in this room in the beginning of September 1806; here they increased considerably in the course of two months, but were not heard or seen after the fire was removed. Their disappearance led me to conclude, that the cold had killed them; but in this I was mistaken: for a brisk fire being kept up for a whole day in the winter, the warmth of it invited my colony from their hiding-place, but not before the evening; after which they continued to skip about and chirp the greater part of the following day, when they again disappeared, being compelled by the returning cold to take refuge in their former retreats. They left the chimney corner on the 28th of May, 1807, after a fit of very hot weather, and revisited their winter residence on the 31st of August. Here they spent the autumn merely, and lie torpid at present in the crevices of the chimney, with the exception of those days, on which they are recalled to a temporary existence by the comforts of a fire.

Crickets are commonly supposed to be exempted by nature from the hardships of torpidity; but the preceding narrative proves the exemption to be conditional in these insects; and those who take the liberty to argue from analogy will feel an inclination, to attribute the same accommodating faculty to other animals, some of which are nearly connected with the welfare of society. In reality, the supposition is strongly favoured by facts: for we have frequent instances in this part of the nation, of sheep living three or four weeks under drifts of snow, where they can procure little or no food; and a ewe was recovered alive from a drift at Ennerdale in Cumberland, on Christmas-day last, after remaining under it five weeks in a space not exceeding one yard in diameter. If the same or any other sheep were confined half the time in a moderately warm room, with but one square yard of grass, no doubt could be entertained respecting the event of the experiment.

Sheep can live long under snow.

Much has been said respecting the torpidity of those birds which

A remark on

birds of passage.

which are seen in summer only; but though the opinion has had its advocates as long ago as Pliny, it has never been proved, and perhaps it never will. For since the cricket avoids the cold when it can, and the woodcock, as well as the snipe, retires from the north at the end of autumn with the same intention; it is highly probable, that the swallow, with many more periodical birds, quits this country, and flies to warmer regions on the approach of winter; while the bat, the dormouse, and hedgehog, are obliged to abide the rigours of the season, benumbed by the frost and debilitated by hunger. But it is time to return from this digression, and to come to the second objection, the proof of which is contained in the following experiment.

The second objection exemplified by two kinds of snails.

I took several specimens of the garden snail, *helix hortensis*, and shut them up in a perforated wafer box; which secluded them from food and water, but not from air. A number of the *helix zonaria* was treated in the same manner; and a few of this species were put into a bottle, which was corked, to cut off all communication with the atmosphere, as well as food and water. Those snails did not live long which were deprived of air; but the specimens of both species did not die which were confined in the perforated boxes. On the contrary they retired into their shell, closing the apertures of them with thin membranes; here they remained dead to all appearance, as long as I kept them dry. But this death was nothing more than apparent; for I restored my prisoners to life in succession, by dropping them into a glass containing water of the temperature of 70° or 72°: after leaving them four or five hours in this situation, I constantly found them alive, and sticking to a plate which covered the vessel. A large garden snail supported this severe confinement nearly three years, being apparently dead all the time; after which it revived upon being put into water, like the rest of its fellow captives.

This wonderful faculty however is not possessed by snails of every description; this I discovered, by treating an aquatic species, the *helix putris*, in the manner described above. The preceding experiment was made in consequence of a short memoir which I met with some years ago, in a volume of the Philosophical Transactions of an older date. The writer

writer of this paper had observed accidentally, that some snails, which had been long confined in a drawer, were found to be alive after being immersed in water: the fact appeared very singular to me, and I was desirous to ascertain the accuracy of it more correctly by a direct experiment.

The proof of the second objection being now finished, I am obliged by want of room to defer the remaining two to a future opportunity.

JOHN GOUGH.

Middleshaw, 5*th Feb.* 1808,

SIR,

I Had the honour of presenting the following memoir to the Society of Nat. Hist. Edinburgh, in October, 1798; since which time it has come to my knowledge, that this learned body is not in the habit of publishing its papers; and as the essay promises to establish the third and fourth objections offered in my last letter to the received theory of torpidity, I have transmitted it to your valuable Journal.

And remain, &c.

JOHN GOUGH.

On the changes produced in the habits of animals by difference of diet and other causes, together with the history of a domesticated dormouse.

The remarks contained in the present essay are not the result of experiments instituted either to confirm or contradict any notion; but were collected from observations made on the general economy of the little quadruped under consideration. Introductory remark.

Having procured two dormice, *mures avellanarii*, in January, 1792, which were caught in the woods but a few days before they came into my hands, I confined them in a cage furnished with a thermometer, and placed in a chamber where no fire was kept. In this situation they were supplied regularly with water and food, consisting of hazel-nuts and biscuit. The weather in February being warm for the sea- Manners of a pair of dormice recently caught.

son

son at the beginning and end of the month, and frosty from the 16th to the 25th, I had an opportunity to observe, that, whenever the thermometer, which was attached to the cage, fell to 42°, the dormice became inactive, and remained apparently insensible as long as the heat of that part of the chamber did not exceed the temperature here specified: but as oft as the mercury reached 47°, they became very susceptible of external impressions, and awaked in the evenings, when they repaired to their stock of provisions, of which they consumed not a little. The same dry food was injudiciously persisted in through the succeeding summer; in consequence of which they grew sickly, and died before the winter commenced: so that I had not a second opportunity to attend to the economy of this couple during the cold season.

The pair killed by improper treatment.

A third dormouse more judiciously treated.

About the middle of April, 1793, I obtained a third dormouse fresh from the woods: former experience taught me to manage this in a manner more congenial to its constitution; for in addition to the nuts and biscuit, it was constantly supplied with green hazel-buds or raisins in spring; with ripe fruits, particularly cherries and pears, in summer; and with apples and raisins in winter. This generous diet not only preserved the creature in health and high condition, but appeared to fortify it against the benumbing effects of cold, which it supported the following winter much better than the other couple had done formerly: for it never slept more than 48 hours, and that but seldom, without visiting the cup which contained its provisions.

Proof of the third objection.

I now began to suspect the torpidity of the dormouse in a wild state, to be nothing but a custom imposed by necessity on a constitution, which nature has intended to retain life during the cold season of winter, with but little food and an imperfect degree of respiration, as well as a languid or perhaps a partial action of the sanguiferous system. The preceding supposition can alone reconcile the difference of manners observable in the dormice I had in 1792, and that which has been described above: for as soon as the necessity of sleeping was removed, the propensity to become torpid with cold disappeared in a great measure. The uncommonly severe weather which ushered in the next year, viz.

1795,

1795, confirmed the foregoing opinion apparently beyond exception: for a constant use of a generous and plentiful diet had by this time completely conquered the torpid habit, which the animal in all probability contracted in its native habitation from hunger, or more properly from a state of inactivity voluntarily imposed on itself, with a view to husband its stock of nuts, which would be frequently too soon exhausted but for this precaution. Notwithstanding the hard frost of January, it braved the cold with wonderful fortitude, or if the expression be thought less exceptionable, with wonderful indifference; for it awaked every evening, when it consumed in the course of the night a quantity of food amounting to 100 or 120 grains, and frequently gnawed the ice which covered the water in the cage: it even undertook, in the coldest part of the month, to repair its nest, which happened to receive an injury, and perfected the task in one night.

Instances of animals changing their habits

Many instances are recorded of animals being compelled by strong circumstances to relinquish their characteristic manners, in order to act a part contrary in several important points to the uniform conduct of their species. Linnæus has preserved the memory of a tame fieldfare, *turdus pilaris*, belonging to a vintner in Stockholm, which learned to drink wine, and became bald in consequence of this strange beverage. I also knew a mastiff, which was equally fond of ale, and never failed to get drunk when an opportunity offered. The hyæna lives on the roots of fritillary, in the unfrequented parts of Africa; but in the vicinities of populous cities it changes into a disgusting glutton, feeding on filth and carrion. May not the nasty ways of the domestic hog be considered as so many new habits introduced by similar causes in lieu of the cleaner manners of the wild animal? The pied flycatcher, *muscicapa atricapilla*, lives on soft seeds and insects in this country; but its food is very different in Norway, especially during winter, when it repairs to the habitations of men, where it subsists on flesh dried in the smoke. Signior Spallanzani converted a pigeon, which is granivorous, into a carnivorous bird, by inducing it in the first place to eat fresh meat, and afterward to give a preference to putrid animal substances. In reality, the facts

facts which prove how little philosophers know of the principle of accommodation, that regulates the animal economy according to prevailing circumstances, are already numerous, and observation bids fair to multiply them.

The diminished action of the brain the cause of torpidity.

I have shown in the present essay, that a quadruped remarkable for its torpidity may be rendered active at all seasons by a plentiful and generous diet: perhaps a contrary regimen properly managed might incline an animal, no less remarkable for its activity, to become torpid at times. The preceding suggestion will not appear absurd to those, who view torpidity in the light it is here represented, I mean as a periodical custom of prolonging sleep to an unusual length, the respiration becoming at the same time slow and feeble, and the heat of the body diminishing of consequence. Some singular anomalies in the history of man himself may be said to answer in part to the foregoing description, and to indicate an incipient propensity to become torpid under certain circumstances. There are instances of great insensibility arising from the operation of causes on the system, which have an evident tendency to destroy the vital power; or which, to speak more properly, incapitate the brain to generate this power in sufficient quantity, to supply the various demands of the voluntary and involuntary functions: the little that is produced being expended on those operations of the economy, which are absolutely necessary for the continuance of life.

Proof of objection 4th.

Dr. Plot relates the case of a poor girl eight years old, who, being beaten by a severe stepmother, and then sent hungry with some refreshments to her father in the fields, could not refrain from eating part of them; reflecting afterwards on the probable consequences of her conduct, she proceeded no further on her way, but retired to a neighbouring wood, and there fell into a profound sleep, being oppressed with fear and sorrow: in this state she remained for seven days, and, when discovered, showed no symptoms of life, beside the softness of her flesh, and flexibility of her joints. Dan. Ludovicus, from whom Dr. Plot borrows this relation, happened to be present, and succeeded in his attempts to recover this poor creature. He first washed a glutinous phlegm from her face with warm water, and cleared her mouth and nostrils from

from a viscid substance that obstructed them: a few spoonfuls of brandy were then administered; after the second she was heard to groan, after the third she opened her eyes, and so came at length to herself by degrees (History of Staffordshire, chap. viii, sect. 36). The same author has also preserved another instance of a sleeper in the circle of his own acquaintance. This is the history of Mary Foster, of Admaston; but her singular case is too imperfectly stated, to ascertain any thing more than the fact and cause of it. She remained in a profound sleep for fourteen days and nights, after an equal period of fear and anxiety, occasioned by the woman falling casually into a well; and the accident seems to have produced in her a disposition to torpor: for two years afterwards she slept two nights and a day at Uttoxeter, but the reason of this relapse is omitted. The annals of medicine furnish without doubt many more examples of a like nature; but the few which I have specified appear sufficient to prove, that torpidity is a mere habit, and not a constitutional principle of the animal economy.

Supplementary remarks.

An experiment by M. Pallas.

I was unacquainted, at the date of the preceding essay, with an experiment made by Mr. Pallas, and mentioned by Mr. Cox, in his Travels through Russia. This celebrated Russian naturalist conquered the torpid habit in a marmot, by confining it through winter to a warm stove, and giving it a plentiful supply of food. If my recollection be correct, the species of Mr. Pallas's marmot is overlooked by Mr. Cox, but the omission is of little moment, seeing the fact has been ascertained by a philosopher of high reputation.

The natural history of the earless marmot, *arctomys citillus*, also establishes the general proposition, viz. that torpidity is a habit, and not a necessary propensity. These animals imitate the manners of the hearth cricket; for those that burrow in the fields fall asleep about the end of September, and appear again with the first symptoms of spring; but when the same quadruped finds its way into a granary, it remains active all winter.

General remark.

The preceding observations agree very well with the substance of the present essay, and my last on the same subject:

ject: but the experiment, made on the dormouse, appears to throw a light on the nature of torpidity; which perhaps, as far as I know, can not be derived from any other fact in natural history: for according to it, a liberal use of nutritious food will in time enable this little animal to support a degree of cold much severer than that which benumbs the same creature when wild and habituated to a meager diet. This is a solitary instance of the surprising effects produced on the constitution by regimen; from which we may infer, that the torpidity of the dormouse arises from the united operations of cold and hunger; but future observations must determine how far other torpid animals are influenced by diet, before we can pronounce the preceding explanation of torpidity to be general.

II.

On the Nonexistence of Oxigen and Hidrogen, as Bases of particular Gasses; the Action of Galcanism; and the compound Nature of the Matter of Heat. In a Letter from G. S. GIBBES, *M. D.*

To Mr. NICHOLSON,

SIR, *Bath, Jan.* 13, 1808.

Objections to the theory of Lavoisier.

YOU have already done me the honour of publishing in your excellent Journal some opinions, which I maintain, respecting the nonexistence of oxigen and hidrogen; and the consequent failure of the Lavoisierian theory of chemistry in explaining the phenomena, which are presented in that science. I now take the liberty of sending some farther observations on the same subject, which lead me to conclude, that my former opinions were well founded, and that the generally received doctrine of the decomposition of water is not consistent with fact. I contend, that in no one experiment have we the least evidence, that the ponderable parts of oxigen and hidrogen air are substances differing from each other, or in any respect peculiar substances; or that water is a compound resulting from the union of these two substances.

Theory of the composition of water not founded on fact.

If

If this positio can be proved, the Lavoisierian theory will lose its fundamental support, and the whole superstructure falls to the ground.

It is asserted, that the phenomena of galvanism, like electricity, are owing to the presence or absence of one and the same fluid, which constitute the positive and negative sides. Phenomena of galvanism produced by one fluid.

If two bodies, acting upon a third produce different effects, the bodies themselves must be different. This contradicted by facts.

A different power is conducted into the water by the two ends of the galvanic battery; for, as the two pieces of platina remain unaltered, the effect on the water in the galvanic experiment must be produced by two different powers, to which the pieces of platina merely act as conductors. The simple fact is then, that the one platina wire produces, when placed in water, one particular air; and the other platina wire, placed under similar circumstances, a different one: the two powers therefore, conducted by the platina wires, must be different.

Bodies in assuming an aeriform state require the union of different other bodies to constitute those characters which distinguish them: therefore these two different airs must have received from the two platina wires two different powers, to enable them, since water is concerned in the production of both, to assume two different aeriform states. The two airs, so formed, have certainly distinguishing characters; for the one supports combustion, and the other is a combustible body. The same substance converted into different gasses, by the addition of different substances.

Water then is by the union of these two galvanic powers transformed into two aeriform bodies, in which reside all the requisite circumstances of inflammation and combustion. Upon this combustion water is reproduced, and the two galvanic powers form fire; fire therefore is composed of the two galvanic powers. Fire a compound of the two galvanic powers.

Water then and one power of the pile produces oxigen air; and water and the other power, hidrogen air: and combustion is always produced by the union of these two powers. The positive end of the galvanic battery then we assert, produces in every instance that effect on bodies which oxigen is asserted to do; and is not the basis or ponderable part of the air, Water with one of these forms oxigen, with the other hidrogen.

air, but the expansible power, which causes water to assume that peculiar aeriform state. The same reasoning holds good with respect to the galvanic property of the negative end of the pile, as in the instance of metallic calces being reducible to their metallic state; and we account for this by saying, that the oxidated or positive state of the metal is destroyed by its being saturated with the hidrogenous or negative power of the pile. In short, bodies are burnt by the power or principle which comes from one end of the pile, and unburnt by the power or principle which comes from the other end of the pile.

Metallic calces reduced by one power saturating the other.

Bodies burnt by one and unburnt by the other.

Metals rendered more combustible by the negative end of the pile:

Metals are combustible bodies, and in becoming oxides they are burnt. Metals not easily burnt are rendered more combustible by being connected with the negative end of the pile. Thus copper, which is easier converted into an oxide than silver, will in ordinary cases take the acid from a solution of silver in nitrous acid, and the silver will be deposited in its metallic form; but if silver be rendered more combustible by being connected with the pile, it will then supersede the copper in its attraction for the acid, and the copper will be deposited in its metallic form. The above proves, that a real and distinct power is communicated to the silver by the pile.

and thus their affinities changed.

Neutral salts decomposed by galvanism.

Mr. Davy has shown, that neutral salts are decomposed by the powers of the pile; that the acids appear on the positive side, and the bases on the negative; and that, when muriatic salts are decomposed, the oxigenated muriatic acid appears on the positive side. The galvanic apparatus resolves the matter of heat into its two constituent principles, which principles, being thereby freed from their affinity with each other, are at liberty to enter into new combinations; these combinations of the one, as with water in oxigen air, in acids, metallic oxides, &c.; and of the other in combustible bodies of all kinds, I shall attempt to illustrate by experiments, which I shall take the liberty of transmitting to you in a future letter.

Galvanic apparatus decomposes the matter of heat into its two principles.

Experiments promised.

I am, Sir,

Yours, &c.

G. S. GIBBES.

III.

III.

Letter from N. R. D., containing some Remarks and Emendations of his Communication in the Number for January.

To Mr. NICHOLSON.

SIR,

I Take the liberty of sending you a few remarks on the translation from Lalande, which you did me the favour of inserting in the last number of your Journal. My only reason for sending it originally to you was, the hopes of being useful, and the same motive induces me to point out the corrections which have occurred to me. Corrections & additions to the paper on the Constellations in our 81st number.

In p. 3 I have given a somewhat different description from Lalande of the means by which we may find the constellation of Gemini; because I think that in general it is much more clear to the beginner, when the object to be found is situate between two others, with which he is already acquainted. I therefore ventured to alter the arrangement of my author's directions, while I preserved the substance of it: but it might have marked the line still more strongly, if I had added with him, that it passes nearly through ε and ζ the two stars in the tail of the great Bear which are nearest the body. Gemini.

P. 5, l. 18. Lalande describes the head of Andromeda as the "most northern" star in the square of Pegasus, and so it really is; but its declination so little exceeds that of β Pegasi, that it would have been much more clear to have called it the "N. E." star. Head of Andromeda.

P. 8. line 6 from bottom. The "leg" of Ophiucus is substituted for the "foot," in consequence of my having used Dr. Bevis's Uranographia Britannica. The eastern foot is there placed in 10° or 11° of south declination. I had not, when I wrote, the opportunity of consulting Flamstead's Atlas Cœlestis, or I should have made no alteration. This circumstance will account for my having omitted the notice of the two feet being on the ecliptic. Ophiucus.

P. 9, l. 12 from bottom. A line drawn from Capella through the Pleiades will also "pass south of α Ceti." It should have been said, as it is in the French, that it will α Ceti.

point

point to α Ceti. The alteration was suggested by looking, through mistake, at the Hyades near Aldebaran, instead of the Pleiades.

α Piscium.

P. 10. The direction for finding α Piscium was altered from the wish before mentioned, of giving two known objects on opposite sides of that which was to be pointed out; and the proximity of o Ceti made it very useful for this purpose. I still think, that this description is better than Lalande's, when o is brilliant; but as that star is sometimes invisible, the original should likewise have been added, which says, that α Piscium will be found in the line drawn from γ, the foot of Andromeda, through the head of Aries.

P. 10, l. 13 from bottom. "Les deux precedentes" are rendered "the two eastern" stars in the body of the great Bear. This translation is only accurate when the constellation is under the pole. The stars should therefore have been described as those which are "farthest from the tail."

Errours in Lalande.

The above remarks may induce your readers to think, that I have taken greater liberty with my original than I have even given notice of in the short note at p. 1: and as it is a bold measure for an anonymous writer, to venture on correcting what has been printed by an author of established fame like Lalande, it may be right to mention a few of the instances which occur in the text, to prove that some revision was necessary. In § 770, Aquila is described as being "au milieu de la Lyre et du Cygne;" there can be no doubt, that this ought to be "au midi de la Lyre et du Cygne." § 774. The tail of the Serpent is said to be "vers l'occident," with respect to Ophiucus, when it certainly is towards the east. § 779. Aquarius is said to be as far from the Dolphin as the Dolphin is from the Eagle; but no one acquainted with the heavens will blame me, for substituting the Lyre in this place instead of the Eagle.—I took considerable pains in comparing the translation with the globe and the Celestial Atlas, and I hope therefore, that it will be found in some parts more accurate even than the original, especially when the following additions are made and errata corrected. I sincerely regret, that there should be any occasion for correction, and I can only apologise by stating,

stating, that the copy was written out under a most unusual press of business, which scarcely allowed me to finish it in time to send it to you as soon as I had promised.

Jan. 15, 1808. N. R. D.

In p. 2, l. 18, *for* points to *read* points nearly to.—p. 4, l. 6, after horns *add* which are 8° apart.—*ib*, l. 32, *for* first *read* third.—p. 7, l. 25, *after* and *add* of the.—p. 8, l. 6, *for* through *read* near: l. 23, *for* south-east *read* south-west: l. 27, *after* θ *add* a changeable star.—p. 9, l. 13 from bott. *for* 3d *read* 2d.—p. 10, l. 7, *for* the Whale *read* Aries.—p. 11, l. 19, *for* 32° 2′ *read* 33° 2′.

IV.

On the Advantages of Grafting Walnut, Mulberry, and Chesnut Trees. By Thomas Andrew Knight, *Esq. F.R.S. &c.**

IN the course of very extensive experience in the propagation of apple and pear trees, I found that the detached parts of the bearing branches of old trees of those species, when employed as grafts, never formed what could with propriety be called young trees: the stocks appeared to afford nutriment only; and the new plants retained, in all instances, the character and habits of the bearing branches of which they once formed parts; and generally produced fruit the second or third year after the grafts had been inserted†. Grafts of bearing branches do not form young trees.

I was therefore induced to hope, that the effects of time might be anticipated in the culture of several fruits, the trees of which remain unproductive during many years after they are planted: and that parts of the bearing branches of those, detached Applied to the speedy production of certain fruits.

* From the Trans. of the Horticultural Society. Vol I, p 60.

† Columella appears to have known, that a cutting of a bearing branch did not form a young tree; for speaking of cuttings of the vine (semina) he says, "optima habentur a lumbis; secunda ab humeris; tertia summa in vite lecta, quæ celerrime comprehendunt, et sunt feraciora, sed et quam celerrime senescunt." *De Arboribus, chap.* 3.

detached from the old trees, and employed as grafts, would still retain the character and habits of bearing branches.

Experiment with the walnut.

Having therefore planted in the spring of 1799 some walnut trees, of two years old, in garden pots, I raised them up to the bearing branches of an old walnut tree, by placing them on the top of poles placed in the earth; and I grafted them, by approach, with parts of the bearing branches of the old tree. A union took place during the summer, and in the autumn the grafts were detached from the parent stock. The plants thus obtained were planted in a nursery, and, without any peculiar care or management, produced both male and female blossoms in the third succeeding spring, and have since afforded blossoms every season. The frost has, however, rendered their blossoms, as well as those of other trees in their vicinity, wholly unproductive during the last three years, and in the spring of 1805, almost wholly destroyed the wood of the preceding year.

With the mulberry.

A similar experiment was made in the same year, but under many disadvantages, on the mulberry tree. I had not any young plants of this tree, and therefore could only make the experiment with scions of one year old; and of these I had only two, which had sprung from the roots of a young tree, in the preceding year. These were planted in pots, and raised to the bearing branches of an old tree, in the manner I have already described in speaking of the walnut tree. One of these scions died; the other, which had but very few roots, succeeded; and the young grafted tree bore fruit the third year, and has continued annually productive. In the last spring I introduced it into my vinery, where its fruit ripened, in the greatest state of perfection, in the beginning of the present month, [January, 1807].

Grafting by approach best for them.

Both the walnut and mulberry tree succeed so ill when grafted, unless by approach, that I can scarcely recommend attempts to propagate them in any other way; but when they succeed by other modes of grafting, nearly the same advantages will probably be obtained: the habit of the bearing branch is, however, least disturbed by grafting by approach.

Spanish chesnut succeeds any way.

The Spanish chesnut succeeds readily when grafted in almost any of the usual ways, and when the grafts are taken from

Dr. Herschel on coloured concentric Rings.

Fig. 9.

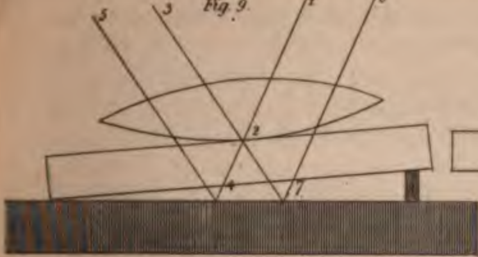


Fig. 10.

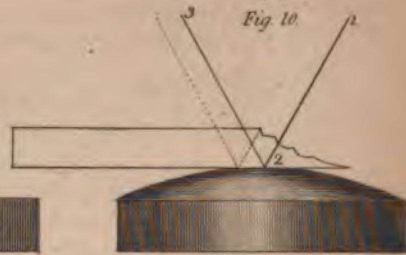


Fig. 11.

Fig. 12.

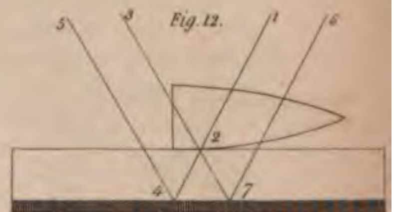
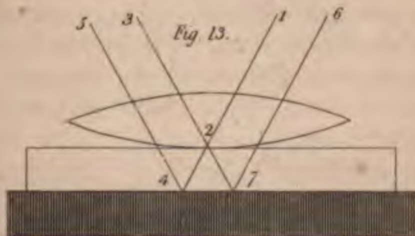


Fig. 13.



Dr. Joseph Reade's Calorimeter.

Fig. 3.

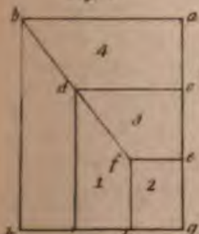


Fig. 2.

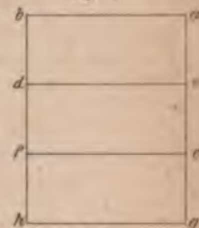


Fig. 1.

from bearing branches, the young trees afford blossoms in the succeeding year: and I am much inclined to think, from experiments I have made on this tree, that by selecting those varieties which ripen their fruit early in the autumn, and by propagating with grafts or buds from young and vigorous trees of that kind, which have just attained the age necessary to enable them to bear fruit, it might be cultivated with much advantage in this country, both for its fruit and timber.

Valuable both for its fruit and timber.

I have tried similar experiments on many other species of trees, and always with the same result; and I entertain no doubt, that the effects of time might be thus anticipated in the culture of any fruit, which is not produced till the seedling trees acquire a considerable age. For I am thoroughly confident, from very extensive and long experience, that the graft derives nutriment only, and not growth, from the young stock in which it is inserted; and that with the life of the parent stock the graft retains its habits and its constitution.

Tried on many other trees, and thus their maturity anticipated.

V.

Experiments for investigating the Cause of the coloured concentric Rings, discovered by Sir Isaac Newton, *between two Object-glasses laid upon one another. By* William Herschel, *L. L. D. F. R. S.*

(Concluded from p. 142.)

XIX. *Of the Place where the different Sets of Rings are to be seen.*

By an application of the same course of the rays, we may now also determine the situation of the place, where the different sets of rings are seen: for, according to what has been said in the foregoing article, the situation of the primary set should be between the lens and the surface of the looking-glass: and the place of the secondary one at the metalline coating of the lowest surface. To try whether this be actually as represented, let us substitute a metalline mirror with

Place where the different sets of rings are seen.

with a slip of glass laid upon it in the room of the piece of looking-glass; and let there be interposed a short bit of wood, one tenth of an inch thick, between the slip of glass and the mirror, so as to keep up that end of the slip which is towards the light. This arrangement is represented in Pl.V, fig. 9, where both sets of rays are delineated. Then if we interpose a narrow tapering strip of card, discoloured with japan ink, between the slip of glass and the mirror, so as to cover it at 7, we do not only still perceive the primary set, but see it better than before: which proves, that, being situated above the slip of glass, the card below cannot cover it. If on the contrary we insert the strip of card far enough, that it may at the same time cover the mirror both at 4 and at 7, we shall lose the secondary set, which proves, that its situation was on the face of the mirror.

Eye-glass requires a different adjustment for each set.

When several sets of rings are to be perceived by the same eye-glass, and they are placed at different distances, a particular adjustment of it will be required for each set, in order to see it well defined. This will be very sensible when we attempt to see three or four sets, each of them situated lower than the preceding; for without a previous adjustment to the distance of the set intended to be viewed, we shall be seldom successful; and this is therefore a corroborating proof of the situation, that has been assigned to different sets of rings.

XX. *Of the Connection between different Sets of Rings.*

Connexion between different sets.

It will now be easy to explain in what manner different sets of rings are connected, and why they have been called primary and dependent. When the incident rays come to the point of contact, and form a set of rings, I call it the primary one: when this is formed, some of the rays are continued by transmission or reflection, but modified so as to convey an image of the primary set with opposite colours forward through any number of successive transmissions or reflections; whenever this image comes to the eye, a set of rings will again be seen, which is a dependent one. Many proofs of the dependency of the second, third, and fourth sets of rings upon their primary one may be given; I shall only mention a few.

Proofs that all

When two sets of rings are seen by a lens placed upon a looking-

looking-glass, the centre of the secondary set will always remain in the same plane with the incident and reflected rays passing through the centre of the primary one. If the point of contact is changed by tilting, the secondary set will follow the motion of the primary set; and if the looking-glass is turned about, the secondary will be made to describe a circle upon that part of the looking-glass, which surrounds the primary one as a centre. If there is a defect in the centre or in the rings of the primary set, there will be exactly the same defect in the secondary one; and if the rays that cause the primary set are eclipsed, both sets will be lost together. If the colour of the primary one is changed, that of the secondary will also undergo its alternate change, and the same thing will hold good of all the dependent rings, when three or four sets of them are seen, that have the same primary one.

the other sets depend on the primary one.

The dependency of all the sets on their primary one may also be perceived, when we change the obliquity of the incident light; for the centres of the rings will recede from one another when that is increased, and draw together when we lessen it, which may go so far, that by an incidence nearly perpendicular we shall bring the dependent sets of rings almost under the primary one.

XXI. *To account for the Appearance of several Sets of Rings with the same coloured Centres.*

It has often happened, that the colour of the centres of differents sets was not what the theory of the alternation of the central colours would have induced me to expect: I have seen two, three, and even four sets of rings, all of which had a white centre. We are however now sufficiently prepared, to account for every appearance relating to the colour of rings and their centres.

Why several sets have the same coloured centre.

Let an arrangement of glasses be as in figure 9. When this is laid down so as to receive an illumination of day light, which should not be strong, nor should it be very oblique, the reflection from the mirror will then exceed that from the surface of glass; therefore the primary set will be seen by the rays 6, 7, coming to the mirror at 7, and going through the point of contact in the direction 7, 2, 3, which proves it to be

 a set

a set that is seen by transmission, and it will therefore have a white centre. The rays 1, 2, 4, passing through the point of contact, will also form a transmitted set with a white centre, which will be seen when the reflection from 4 to 5 conveys it to the eye. But these two sets have no connection with each other; and as primary sets are independent of all other sets, I have only to prove, that this secondary set belongs not to the primary one which is seen, but to another invisible one. This may be done as follows.

Introduce the black strip of card, that has been mentioned before, till it covers the mirror at 7; this will take away the strong reflection of light, which overpowers the feeble illumination of the rays 1, 2, 3; and the real hitherto eclipsed primary set, belonging to the secondary one with a white centre, will instantly make its appearance with a black one. We may alternately withdraw and introduce again the strip of card, and the centre of the primary set will be as often changed from one colour to its opposite; but the secondary set, not being dependent on the rays 6, 7, will not be in the least affected by the change.

If the contact should have been such as to give both sets with orange centres, the introduction of the strip of card will prove, that the set which is primary to the other has really a green centre.

Another way of destroying the illusion is to expose the same arrangement to a brighter light, and at the same time to increase the obliquity of the angle of incidence; this will give a sufficient reflection from the surface of the glass to be no longer subject to the former deceptive appearance; for now the centre of the primary set will be black, as it ought to be.

XXII. *Of the reflecting Surfaces.*

Situation of the surfaces that reflect the rings.

The rays of light, that form rings between glasses, must undergo certain modifications by some of the surfaces through which they pass, or from which they are reflected; and to find out the nature of these modifications, it will be necessary to examine which surfaces are efficient. As we see rings by reflection, and also by transmission, I shall begin with the most simple, and show experimentally the situation

of

of the suface that reflects, not only the primary but also the secondary sets of rings.

Upon a slip of glass, the lowest surface of which was deprived of its polish by emery, I laid an object-glass of 21 feet focal length, and saw a very complete set of rings. I then put the same glass upon a plain metalline mirror, and saw likewise a set of them. They were consequently not reflected from the lowest surface of the subjacent glass or metal.

It will easily be understood, that, were we to lay the same object-glass upon a slip of glass emeried on both sides, or upon an unpolished metal, no rings would be seen. It is therefore neither from the first surface of the incumbent object-glass, nor from its lowest, that they are reflected; for if they could be formed without the modification of reflection from the upper surface of a subjacent glass or metal, they would still be seen when laid on rough surfaces; and consequently, the efficient reflecting surface, by which we see primary sets of rings, is that which is immediately under the point of contact.

The reflecting surface that under the point of contact.

To see a secondary set of rings by reflection, is only an inversion of the method of seeing a primary one. For instance, when a lens is laid upon a looking glass, the course of the rays represented in figure 8, pl. IV, will show, that the rays, 1, 2, 3, 5, 6, by which a secondary set is seen, are reflected about the point of contact at 3, and that the lowest surface of the incumbent lens is therefore the efficient reflecting one; and thus it is proved, that in either case of seeing reflected rings, one of the surfaces that are joined at the point of contact contributes to their formation by a certain modification of reflection.

XXIII. *Of the transmitting Surfaces.*

It would seem to be almost self-evident, that, when a set of rings is seen by transmission, the light which occasions them must come through all the four surfaces of the two glasses which are employed; and yet it may be shown, that this is not necessary. We may, for instance, convey light into the body of the subjacent glass through its first surface, and let it be reflected within the glass at a proper angle, so that it

Transmitting surfaces.

it may come up through the point of contact, and reach the eye, having been transmitted through no more than three surfaces. To prove this I used a small box, blackened on the inside, and covered with a piece of black pasteboard, which had a hole of about half an inch in the middle. Over this hole I laid a slip of glass with a 56-inch lens upon it; and viewed a set of rings given by this arrangement very obliquely, that the reflection from the slip of glass might be copious. Then guarding the point of contact between the lens and the slip of glass from the direct incident light, I saw the rings, after the colour of their centre had been changed, by means of an internal reflection from the lowest surface of the slip of glass; by which it rose up through the point of contact, and formed the primary set of rings, without having been transmitted through the lowest surface of the subjacent glass. The number of transmitting surfaces is therefore by this experiment reduced to three; but I shall soon have an opportunity of showing, that so many are not required for the purpose of forming the rings.

XXIV. *Of the Action of the first Surface.*

Action of the upper surface. We have already shown, that two sets of rings may be seen by using a lens laid upon a slip of glass; in which case, therefore, whether we see the rings by reflection or by transmission, no more than four surfaces can be essential to their formation. In the following experiments for investigating the action of these surfaces I have preferred metalline reflection, when glass was not required, that the apparatus might be more simple.

This not affected by a strong scratch, Upon a plain metalline mirror I laid a double convex lens, having a strong emery scratch on its upper surface. When I saw the rings through the scratch, they appeared to have a black mark across them. By tilting the lens, I brought the centre of the rings upon the projection of the scratch, so that the incident light was obliged to come through the scratch to the rings, and the black mark was again visible upon them, but much stronger than before. In neither of the situations were the rings disfigured. The stronger mark was owing to the interception of the incident light, but when the rings had received

received their full illumination, the mark was weaker, because in the latter case the rings themselves were probably complete, but in the former deficient.

I placed a lens that had a very scabrous polish on one side, but was highly polished on the other, upon a metalline mirror. The defective side being uppermost, I did not find that its scabrousness had any distorting effect upon the rings. scabrousness,

I splintered off the edge of a plain slip of glass; it broke as it usually does with a waving, striated, curved slope coming to an edge. The splintered part was placed upon a convex metalline mirror of 2 inches focus, as in Pl. V, fig. 10. The irregularity of the striated surface, through which the incident ray 1, 2, was made to pass, had very little effect upon the form of the rings; the striæ appearing only like fine dark lines, with hardly any visible distortion; but when, by tilting, the returning ray, 2, 3, was also brought over the striated surface, the rings were much disfigured. This experiment therefore seems to prove, that a very regular refraction of light by the first surface is not necessary; for though the rings were much disfigured, when the returning light came through the splintered defect, this is no more than what must happen to the appearance of every object, which is seen through a distorting medium. or irregularity.

I laid the convex side of a plano-convex lens of 2·8-inch focus with a diameter of 1·5 upon a plain mirror, and when I saw a set of rings, I tilted the lens so as to bring the point of contact to the very edge of the lens, both towards the light and from the light, which, on account of the large diameter of the lens, gave a great variety in the angle of incidence to the rays which formed the rings; but no difference in their size or appearance could be perceived. This seems to prove, that no modification of the first surface in which the angle of incidence is concerned, such as refraction and dispersion, has any share in the production of the rings, and that it acts merely by the intromission of light; and though even this is not without being influenced by a change of the angle, it can only produce a small difference in the brightness of the rings. Altering the angle of incidence had no effect.

A more forcible argument, that leads to the same conclusion, Farther proof.

sion, is as follows. Laying down three 54-inch double convex lenses, I placed upon the first the plain side of a plano-convex lens of $\frac{1}{4}$ inch focus; upon the second, a plain slip of glass; and upon the third, the plain side of a plano-concave lens also $\frac{1}{4}$ inch focus. I had before tried the same experiment with glasses of a greater focal length, but selected these to strengthen the argument. Then, as nothing could be more different than the refraction of the upper surfaces of these glasses, I examined the three sets of rings that were formed by these three combinations, and found them so perfectly alike, that it was not possible to perceive any difference in the size and colour. This shows, that the first surface of the incumbent glasses merely acts as an inlet to the rays that afterward form the rings.

The first surface simply an inlet to the rays.

Surface roughened with emery.

To confirm the idea, that the mere admission of light would be sufficient, I used a slip of glass polished on one side but roughened with emery on the other; this being laid upon a 21-feet object-glass, I saw a set of rings through the rough surface; and though they appeared hazy, they were otherwise complete in figure and colour. The slip of glass, when laid in the same manner upon the letters of a book, made them appear equally hazy; so that the rings were probably as sharply formed as the letters.

Having now already great reason to believe, that no modification, that can be given by the first surface to the incident rays of light, is essential to the formation of the rings, I made the following decisive experiment.

Experimentum crucis.

Upon a small piece of looking-glass I laid half a double convex lens of 16 inches focus, with the fracture exposed to the light, as represented in figure 11. Under the edge of the perfect part of the lens was put a small lump of wax, soft enough to allow a gentle pressure to bring the point of contact towards the fractured edge, and to keep it there. In this arrangement it has already been shown, that there are two different ways of seeing two sets of rings: by the rays 1, 2, 3, we see a primary set; and by 1, 2, 4, 5, the secondary set belonging to it: by the rays 6, 7, 2, 3, we see a different primary set; and by 6, 7, 2, 4, 5, we see its secondary one. That this theory is well founded has already been proved; but

but if we should have a doubt remaining, the interposition of any small opaque object upon the looking glass near the fracture will instantly stop the latter two sets of rings, and show the alternate colours of the two sets, that will then be seen by the rays 1, 2, 3, and 1, 2, 4, 5. Remove in the next place the stop from the looking-glass, and bring the second shadow of the penknife over the primary set, and there will then only remain the two sets of rings formed by incident rays which come from 6, and which have never passed through the upper surface of the lens. Now, as both sets of rings in this case are completely formed by rays transmitted upwards from the coated part of the looking-glass without passing through the first surface of the incumbent lens, the proof that the modifying power of that surface is not required to the formation of the rings, is established.

It can hardly be supposed, that the first surface of the lens should have any concern in the formation of the rings, when the rays are reflected from the looking-glass towards the eye; but the same experiment, that has proved that this surface was not required to be used with incident rays, will show, that we may do without it when they are on their return. We need only invert the fractured lens, as in figure 12, when either 1, 2, 4, 5, or 6, 7, 2, 4, 5, will convey the image of the rings, after their formation, to the eye, without passing through any part of the lens. The upper surface not necessary for the returning ray.

XXV. *Of the Action of the second Surface.*

As rings are formed when two glasses are laid upon each other, it is but reasonable to expect, that the two surfaces at least which are placed together should have an immediate effect upon them; and so much the more, as it has been ascertained, that the first surface assists only by permitting light to pass into the body of the glass. Some of the experiments, that have been instituted for examining the action of the first surface, will equally serve for investigating that of the second. Action of the second surface.

The lens already used with a strong emery scratch being again placed on the mirror, but with the injured side downwards, I found that the rings, when brought under the scratch, Scratched.

were

were not distorted; they had only a black mark of the same shape as the scratch across them.

Scabrous.

The lens with a scabrous side was also placed again upon the mirror, but with the highly polished side upwards. In this position the scabrousness of the lowest surface occasioned great irregularity among the rings, which were indented and broken wherever the little polished holes that make up a scabrous surface came near them; and if by gently lifting the lens a strong contact was prevented, the colours of the rings were likewise extremely disfigured and changed.

Is the distortion occasioned by the rings being seen through an irregular medium?

As we have now seen that a polished defect upon the second surface will affect the figure of the rings that are under it, it will remain to be determined, whether such defects do really distort them by some modification they give to the rays of light in their passage through them, or whether they only represent the rings as deformed, because we see them through a distorted medium. For although the scabrousness did not sensibly affect the figure of the rings when it was on the first surface, we may suppose the little polished holes to have a much stronger effect in distorting the appearance of the rings when they are close to them. The following experiment will entirely clear up this point.

Effect of a polished line.

Over the middle of a 22-inch double convex lens I drew a strong line with a diamond, and gave it a polish afterward, that it might occasion an irregular refraction. This being prepared, I laid a slip of glass upon a plain metalline mirror, and placed the lens with the polished line downwards upon the slip of glass. This arrangement has been shown to give two sets of rings. When I examined the primary set, a strong disfiguring of the rings was visible; they had the appearance of having been forced asunder, or swelled out, so as to be much broader one way than another. The rings of the secondary set had exactly the same defects, which, being strongly marked, could not be mistaken. The centres of the two sets, as usual, were of opposite colours, the first being black, the second white; and all those defects, that were of one colour in the first set, were of the opposite colour in the second. When, by the usual method, I changed the colours of the centres of the rings, making that of the primary white

The primary set much disfigured; and likewise the secondary.

and

and of the secondary black, the defects in both sets were still exactly alike, and as before; except that they had also undergone the like transformation of colour, each having assumed its opposite. It remains now only to show, that this experiment is decisive; for by the established course of the rays we saw the secondary set of rings when it had a white centre by the transmitted rays marked 1, 2, 4, 5, in figure 13; and when it had a black one, by the reflected rays 6, 7, 2, 4, 5, of the same figure; but in neither of these two cases did the rays come through the defective part of the lens in their return to the eye.

This proves it to be concerned in the formation of the rings.

This experiment proves more than we might at first be aware of; for it does not only establish, that the second surface, when properly combined with a third surface, has a modifying power, whereby it can interrupt the regularity of the rings, but also one whereby it contributes to their formation; for, if it can give an irregular figure to them by transmitting its irregularly modified rays, it follows, that when these rays are regularly modified it will be the cause of the regular figure of the rings. Nay, it proves more; for if it modifies the figure of the rings by transmission, it modifies them no less by reflection; which may be seen by following the course of the rays 6, 7, 2, 4, 5; for as they do not pass through the defective place of the lens, they can only receive their modification from it by reflection. This opens a field of view to us, that leads to the cause of all these intricate phenomena, of which in a second part of this paper I shall avail myself.

Hence we may be led to the cause of the phenomena.

XXVI. *Of the Action of the third Surface.*

Action of the 3d surface.

When a double convex lens is laid upon a plain metalline mirror, that happens to have an emery scratch in its surface, we see it as a black line under the rings that are formed over them. This shows, that, when a defect from want of polish has not a power to reflect light in an irregular manner, it cannot distort the rings that are formed upon it.

Defects in this capable of distorting the rings,

When I laid a good 21-feet object glass upon a plain slip that had some defects in its surface, the rings, in every part of the object glass that was brought over them, were always disfigured; which proves, that a reflection from a defective third

third surface has a power of forming distorted rings, and that consequently a reflection from one that is perfect must have a power of forming rings without distortion, when it is combined with a proper second surface.

both of the primary and secondary sets. When the defective slip of glass, with a perfect lens upon it, was placed upon a metalline mirror, I saw the secondary set affected by distortions of the rings that were perfectly like those in the primary set; which proves, that a polished defect in the third surface will give modifications to the rays that form the rings by transmission as well as by reflection.

XXVII. *The Colour of the reflecting and transmitting Surfaces is of no consequence.*

The colour of the surfaces of no consequence. I laid seven 54-inch double convex lenses upon seven coloured pieces of plain glass. The colours of the glasses were those which are given by a prism, namely, violet, indigo, blue, green, yellow, orange, and red. The rings reflected from each of these glasses were in every respect alike; at least so far that I could have a black, a white, a red, an orange, a yellow, a green, or a blue centre with every one of them, according to the degree of pressure I used. The lenses being very transparent, it may be admitted, that the colours of the glasses seen through them would in some degree mix with the colours of the rings; but the action of the cause that gives the rings was not in the least affected by that circumstance.

I saw the rings also by direct transmission through all the coloured glasses except a dark red, which stopped so much light, that I could not perceive them. The colour of the glasses, in this way, coming directly to the eye, gave a strong tinge to the centres of the rings, so that instead of a pure white I had a blueish white, a greenish white, and so of the rest; but the form of the rings was no less perfect on that account.

XXVIII. *Of the Action of the fourth Surface.*

Action of the 4th surface. We have already seen, that a set of rings may be completely formed by reflection from a third surface, without the introduction of a fourth; this, at all events, must prove, that

that such a surface is not essential to the formation of rings; but as not only in direct transmission, but also when two sets of rings are to be seen, one of which may be formed by transmission, this fourth surface must be introduced; I have ascertained by the following experiments how far the same has any share in the formation of rings.

In direct transmission, where the light comes from below, the fourth surface will take the part which is acted by the first, when rings are seen reflected from a metalline mirror. Its office therefore will be merely to afford an entrance to the rays of light into the substance of the subjacent glass; but when that light is admitted through the first, second, and third surfaces, the fourth takes the office of a reflector, and sends it back towards the point of contact. It will not be required to examine this reflection, since the light thus turned back again is, with respect to the point of contact, in the same situation in which it was after its entrance through the first surface, when it proceeded to the same point; but when two sets of rings are to be formed by rays, either coming through this point directly towards the fourth surface, or by reflection from the same point towards the place where the secondary rings are to be seen, it will then be necessary to examine, whether this surface has any share in their formation, or whether these rings, being already completely formed, are only reflected by it to the eye. With a view to this, I selected a certain polished defect in the surface of a piece of coach-glass, and when a 26-inch lens was laid upon it, the rings of the set it produced were much distorted. The lens was then put upon a perfect slip of glass, and both together were then laid upon the defective place of the coach-glass. The rings of the secondary set reflected by it were nevertheless as perfect as those of the primary set. It occurred to me, that these rings might possibly be reflected from the lowest surface of the perfect slip of glass, especially as by lifting it up from the coach-glass I still continued to see both sets. To clear up this point, therefore, I took away the slip, and turning the defective place of the coach-glass downwards, produced a set of perfect rings between the lens and the upper surface of the coach-glass, and brought it into such a situation, that a secondary set must be

Experiment with a polished defect in this surface.

be reflected from the defective place of the lowest surface. This being obtained, the rings of this set were again as well formed, and as free from distortions, as those of the primary set.

Refraction of the 4th surface has little or no effect.

Upon a plain metalline mirror I laid down two lenses, one a plano-convex, the other a plano-concave, both of 2·9 inches focus, and having the plain side upwards. When two 21-inch double convex glasses were laid upon them, the secondary sets of both the combinations were of equal size, and perfectly like their primary sets; which proves, that the refraction of the fourth surface is either not at all concerned, or at least has so little an effect in altering the size of the rings that it cannot be perceived.

The result of the foregoing experiments, relating to the action of the several surfaces, is,

General results.

I. That only two of them are essential to the formation of concentric rings.

II. That these two must be of a certain regular construction, and so as to form a central contact.

III. That the rays from one side, or the other, must either pass through the point of contact, or through one of the surfaces about the same point to the other to be reflected from it.

IV. And that in all these cases a set of rings will be formed, having their common centre in the place where the two surfaces touch each other.

XXIX. *Considerations that relate to the Cause of the formation of concentric Rings.*

Inquiry concerning the cause of the rings.

It is perfectly evident, that the phenomena of concentric rings must have an adequate cause, either in the very nature or motion of the rays of light, or in the modifications that are given to them by the two essential surfaces that act upon them at the time of the formation of the rings.

This seems to reduce the cause we are looking for to an alternative, that may be determined: for if it can be shown, that a disposition of the rays of light to be alternately reflected and transmitted cannot account for the phenomena, which this hypothesis is to explain, a proposition of accounting for them by modifications that may be proved, even on the

the very principles of Sir. I. Newton, to have an existence, will find a ready admittance. I propose, therefore, now to give some arguments, which will remove an obstacle to the investigation of the real cause of the formation of the concentric rings; for after the very plausible supposition of the alternate fits, which agrees so wonderfully well with a number of facts that have been related, it will hardly be attempted, if these should be set aside, to ascribe some other inherent property to the rays of light, whereby we might account for them; and thus we shall be at liberty to turn our thoughts to a cause, that may be found in the modifications arising from the action of those surfaces, which have been proved to be the only essential ones in the formation of rings.

XXX. *Concentric Rings cannot be formed by an alternate Reflection and Transmission of the Rays of Light.*

One of the most simple methods of obtaining a set of concentric rings is, to lay a convex lens on a plain metalline mirror; but in this case we can have no transmission of rays, and therefore we cannot have an alternate reflection and transmission of them. If to get over this objection it should be said, that, instead of transmission, we ought to substitute absorption; since those rays, which in glass would have been transmitted, will be absorbed by the metal, we may admit the elusion: it ought however to have been made a part of the hypothesis.

They cannot be formed by alternate reflection and transmission of rays.

XXXI. *Alternate Fits of easy Reflection and easy Transmission, if they exist, do not exert themselves according to various Thicknesses of thin Plates of Air.*

In the following experiment, I placed a plain well polished piece of glass 5·6 inches long, and 2·3 thick, upon a plain metalline mirror of the same length with the glass; and in order to keep the mirror and glass at a distance from each other, I laid between them, at one end, a narrow strip of such paper as we commonly put between prints. The thickness of that which I used was the 640th part of an inch; for 128 folds of it laid together would hardly make up two tenths. Upon the glass I put a 39-inch double convex lens;

If fits of easy reflection and transmission exist, they do not exert themselves according to various thicknesses of thin plates of air.

lens; and having exposed this combination to a proper light, I saw two complete sets of coloured rings.

Change of thickness in the plate of air capable of affecting the colour, by Sir I. N.'s hypothesis,

In this arrangement, the rays which convey the secondary set of rings to the eye must pass through a thin wedge of air, and if these rays are endowed with permanent fits of easy reflection, and easy transmission, or absorption, their exertion, according to Sir I. Newton, should be repeated at every different thickness of the plate of air, which amounts to the $\frac{1}{88739}$ part of an inch, of which he says, "Hæc est crassitudo aeris in primo annulo obscuro radiis ad perpendiculum incidentibus exibito, qua parte is annulus obscurissimus est." The length of the thin wedge of air, reckoned from the line of contact, to the beginning of the interposed strip of paper, is 5·2 inches, from which we calculate, that it will have the above mentioned thickness at $\frac{1}{27}$ of an inch from the contact; and therefore at $\frac{1}{54}$, $\frac{3}{54}$, $\frac{5}{54}$, $\frac{7}{54}$, $\frac{9}{54}$, $\frac{11}{54}$, &c. we shall have the thickness of air between the mirror and glass equal to $\frac{1}{178000}$, $\frac{3}{178000}$, $\frac{5}{178000}$, $\frac{7}{178000}$, &c.; of which the same author says, that they give "crassitudines aeris in omnibus annulis lucidis, qua parte illi lucidissime sunt." Hence it follows, that, according to the above hypothesis, the rings of the secondary set, which extended over a space of ·14 of an inch, should suffer more than seven interruptions of shape and colour in the direction of the wedge of air.

occurred at least 7 times,

In order to ascertain, whether such an effect had any existence, I viewed the secondary set of rings upon every part of the glass-plate, by moving the convex lens from one end of it gradually to the other; and my attention being particularly directed to the 3d, 4th, and 5th rings, which were extremely distinct, I saw them retain their shape and colour all the time without the smallest alteration.

without producing the effect.

The same experiment was repeated with a piece of plain glass instead of the metalline mirror, in order to give room for the fits of easy transmission, if they existed, to exert themselves; but the result was still the same; and the constancy of the brightness and colours of the rings of the secondary set plainly proved, that the rays of light were not affected by the thickness of the plate of air through which they passed.

XXXII.

XXXII. *Alternate Fits of easy Reflection and easy Transmission, if they exist, do not exert themselves according to various Thicknesses of thin Plates of Glass.*

I selected a well polished plate of coach glass 17 inches long, and about 9 broad. Its thickness at one end was 33, and at the other 31 two hundredths of an inch; so that in its whole length it differed $\frac{1}{100}$ of an inch in thickness. By measuring many other parts of the plate I found, that it was very regularly tapering from one end to the other. This plate, with a double convex lens of 55 inches laid upon it, being placed upon a small metalline mirror, and properly exposed to the light, gave me the usual two sets of rings. In the secondary set, which was the object of my attention, I counted twelve rings, and estimated the central space between them to be about $1\frac{1}{2}$ times as broad as the space taken up by the 12 rings on either side; the whole of the space taken up may therefore be reckoned equal to the breadth of 40 rings of a mean size: for the 12 rings, as usual, were gradually contracted in breadth as they receded from the centre, and, by a measure of the whole space thus taken up, I found, that the breadth of a ring of a mean size was about the 308th part of an inch.

No fits exert themselves according to various thicknesses of plates of glass.

Now, according to Sir I. Newton's calculation of the action of the fits of easy reflection and easy transmission in thick glass plates, an alternation from a reflecting to a transmitting fit requires a difference of $\frac{1}{137545}$ part of an inch in thickness*; and by calculation this difference took place in the glass plate that was used at every 80th part of an inch of its whole length; the 12 rings, as well as the central colour of the secondary set, should consequently have been broken by the exertion of the fits at every 80th part of an inch; and from the space over which these rings extended, which was about ·13 inch, we find that there must have been more than ten such interruptions or breaks in a set of which the 308th part was plainly to be distinguished. But when I drew the glass plate gently over the small mirror, keeping

* Newton's Optics, p 277.

the secondary set of rings in view, I found their shape and colour always completely well formed.

This experiment was also repeated with a small plain glass instead of the metalline mirror put under the large plate. In this manner it still gave the same result, with no other difference but that only six rings could be distinctly seen in the secondary set, on account of the inferior reflection of the subjacent glass.

XXXIII. *Coloured Rings may be completely formed without the Assistance of any thin or thick Plates, either of Glass or of Air.*

The rings may be formed without thick or thin plates of glass or air.

The experiment I am now to relate was at first intended to be reserved for the second part of this paper, because it properly belongs to the subject of the flection of the rays of light, which is not at present under consideration; but as it particularly opposes the admission of alternate fits of easy reflection and easy transmission of these rays in their passage through plates of air or glass, by proving, that their assistance in the formation of rings is not required, and also throws light upon a subject, that has at different times been considered by some of our most acute experimentalists, I have used it at present, though only in one of the various arrangements, in which I shall have occasion to recur to it hereafter.

Experiment of Sir I. Newton,

Sir I. Newton placed a concave glass mirror at double its focal length from a chart, and observed, that the reflection of a beam of light admitted into a dark room, when thrown upon this mirror, gave "four or five concentric irises or "rings of colours like rainbows *." He accounts for them by alternate fits of easy reflection and easy transmission exerted in their passage through the glass plate of the concave mirror †.

of the duke of Chaulnes,

The Duke de Chaulnes concluded from his own experiments of the same phenomena, "that these coloured rings depended upon "the first surface of the mirror, and that the "second surface, or that which reflects them after they had "passed the first, only served to collect them and throw them

* Newton's Optics, p. 265 † Ibid, p. 277.

"upon

"upon the pasteboard, in a quantity sufficient to make them "visible *."

Mr. Brougham, after having considered what the two authors I have mentioned had done, says, "that upon the whole "there appears every reason to believe, that the rings are "formed by the first surface out of the light, which, after "reflection from the second surface, is scattered, and passes "on to the chart †." of Brougham,

My own experiment is as follows. I placed a highly polished 7 feet mirror, but of metal instead of glass, that I might not have two surfaces, at the distance of 14 feet from a white screen, and through a hole in the middle of it one tenth of an inch in diameter I admitted a beam of the sun into my dark room, directed so as to fall perpendicularly on the mirror. In this arrangement the whole screen remained perfectly free from light, because the focus of all the rays, which came to the mirror, was by reflection thrown back into the hole, through which they entered. When all was duly prepared, I made an assistant strew some hair-powder with a puff into the beam of light, while I kept my attention fixed upon the screen. As soon as the hair-powder reached the beam of light, the screen was suddenly covered with the most beautiful arrangement of concentric circles, displaying all the brilliant colours of the rainbow. A great variety in the size of the rings was obtained by making the assistant strew the powder into the beam at a greater distance from the mirror: for the rings contract by an increase of the distance, and dilate on a nearer approach of the powder. of the author.

This experiment is so simple, and points out the general causes of the rings, which are here produced, in so plain a manner, that we may confidently say they arise from the flection of the rays of light on the particles of the floating powder, modified by the curvature of the reflecting surface of the mirror.

Here we have no interposed plate of glass of a given thickness between one surface and another, that might pro-

* Priestley's History, &c. on the Colours of thin Plates, p. 515.

† Phil. Trans. for 1796, p. 216.

duce the colours by reflecti[illegible]ome rays of light and transmitting others; and if we were inclined to look upon the distance of the particles of the floating powder from the mirror as plates of air, it would not be possible to assign any certain thickness to them, since these particles may be spread in the beam of light over a considerable space, and perhaps none of them will be exactly at the same distance from the mirror.

I shall not enter into a further analysis of this experiment, as the only purpose for which it is given in this place is to show, that the principle of thin or thick plates, either of air or glass, on which the rays might alternately exert their fits of easy reflection and easy transmission, must be given up, and that the fits themselves of course cannot be shown to have any existence.

XXXIV. *Conclusion.*

Newton's theory of the size, and interstices of bodies, founded on fits of easy reflection and transmission of light, unsupported by fact.

It will hardly be necessary to say, that all the theory relating to the size of the parts of natural bodies and their interstices, which Sir I. Newton has founded upon the existence of fits of easy reflection and easy trasmission, exerted differently, according to the different thickness of the thin plates of which he supposes the parts of natural bodies to consist, will remain unsupported; for if the above mentioned fits have no existence, the whole foundation, on which the theory of the size of such parts is placed, will be taken away; and we shall consequently have to look out for a more firm basis, on which a similar edifice may be placed. That there is such a one we cannot doubt; and what I have already said will lead us to look for it in the modifying power, which the two surfaces, that have been proved to be essential to the formation of rings, exert upon the rays of light. The Second Part of this Paper, therefore, will enter into an examination of the various modifications, that light receives in its approach to, entrance into, or passage by, differently disposed surfaces or bodies; in order to discover, if possible, which of them may be the immediate cause of the coloured rings that are formed between glasses.

VI.

Description of a newly invented Calorimeter; with Experiments to prove, that an increased Capacity for Caloric accompanies an Increafe of Temperature. By JOSEPH READE, *M. D.*

SIR, *Edinburgh, Jan.* 22, 1808.

I Beg leave to communicate, through the medium of your very interesting and scientific Journal, the invention of a calorimeter, free from those inaccuracies incident to the apparatus of Messrs. Lavoisier and Laplace, in which it was impossible to guard against errours arising from capillary attraction, from the process of freezing and thawing proceeding at the same period, and likewise from the influence of a current of atmospheric air. In this communication I will confine myself to a summary description of the apparatus, and of a discovery deduced from it, which must influence in a most important manner, if proved, the investigations of caloric; that, contrary to received opinion, water increases in capacity from the thermometric range of 32 to 212, in a just rate for every degree of temperature communicated.

Defects of the apparatus of Lavoisier and Laplace.

Capacity of water for caloric increases uniformly with its temperature.

Description of the Calorimeter, which is to be formed of thin sheets of Brass or Tin.

The innermost compartment No. 1, Pl. V, fig. 1, designed for the fluid to be subjected to experiment, is to be stopped with a thermometric cork, *a*, or, what is better, a thermometer surrounded with chamois leather, and made to fit accurately the aperture. The second compartment, No. 2, holds a quantity of water, and is likewise to be stopped by a thermometric cork, *b*, made air-tight by sealing wax, as this water is not to be removed from the compartment during the course of the experiments.

The calorimeter describ d.

The external compartment, 3, is designed to act as an imperfect conductor of caloric, and is to have a coating of

list

list or flannel between the sheets of brass, which, combined with the confined air, renders the instrument extremely accurate, a minute elapsing before the thermometer fell 1 degree at 150°. Therefore in experiments scarcely requiring that time, there can be no abstraction of any consequence by the atmosphere.

Method of finding the comparative specific heat of two fluids.

When we wish to estimate the specific caloric of two fluids, suppose oil and water, we bring the calorimeter to the precise temperature of 32°, 40°, 50°, or any other we desire, indicated by the two thermometers. We then fill the interior compartment, No. 1, with water at 212°, and immediately stop it with the thermometric cork, *a*. After agitating the apparatus for about the space of 1½ minute in a horizontal position, the thermometers indicate the rise experienced by the water at 50° in the second compartment, and the number of degrees lost by the water at 212° in the interior. Suppose the calorimeter be raised from 50° to 80°, we take that number as the specific caloric of water. We then pour the water from the interior compartment, and again reduce the temperature of the apparatus to 50°, which is speedily accomplished, by pouring cold water into the innermost compartment, until the thermometers are reduced to the desired point. We are next to fill the interior compartment with oil at 212°; and if, after agitation, on examining the two thermometers, we find the temperature raised, suppose to 60°, we easily find the specific caloric of oil compared with water. Thus taking water as the standard, in a short time all fluids may be examined. By substituting an iron cage, solids may be subjected to experiment; so likewise may fluids, which act chemically on metals, by enclosing them in a glass vessel.

Solids and corrosive fluids may likewise be examined.

The author engaged in a series of experiments.

I am at present engaged in a series of experiments, which I hope soon to be enabled to lay before the public. Here the reader is to take notice, that I have only used ideal numbers, more clearly to illustrate the mode of operating with the apparatus, and by no means indicative of the real specific caloric of oil and water. I will end this part of my communication by remarking, that in this instrument the inaccuracies arising from abstraction of caloric by the atmosphere and vessel are obviated, which was impossible by

means

means of mixture; for in pouring the hot liquid into the interior chamber, the pipe of the kettle may enter it, so as entirely to prevent the abstraction of heat, and the vessel must act in a similar manner on both fluids. Inaccuracies obviated by this apparatus.

It is one of the most important questions in chemistry, to determine, whether or not the capacities of fluids are permanent from 32° to 212°: or in other words, whether 10 degrees of caloric, added to water at 32°, will produce the same elevation of temperature, as 10 degrees thrown into the same quantity at 200°. Most chemists are of opinion, that water changes its capacity at two points only, in passing from the solid to the fluid, and from the fluid to the aeriform state; and consequently, that there is a permanency of capacity between the thermometric range of the freezing and boiling points. Drs. Crawford, Black, Irvine, de Luc, &c. thought they decidedly proved this to be the fact, by a number of experiments; for on mixing equal quantities of water at different temperatures, they found nearly a mean produced. "The air of the room," says Dr. Crawford, "being 61·5°, a quantity of water, weighing 13lbs. 10½oz. was heated in a slight tinned iron vessel, that had a cover of the same metal closely adapted to it, a thermometer being inserted in the centre of the cover by means of a cork. When the water was raised to the desired temperature, it was gently agitated, that every part of it might be brought to the same heat. The thermometer immersed in it pointing precisely to 120·6°, an equal quantity of cold water at 50·9°, the parts of which were also brought by agitation to a common temperature, was mixed with the warm, by pouring it into the tinned vessel in which the latter was contained. When the mixture was reduced, by agitating it with a wooden rod to a mean heat, its temperature at the end of one minute was 89·8°. Allowing therefore ·066° for the heat lost in the first minute, we have 89·866° for the true temperature of the mixture. If the thermometer at the moment of immersion had indicated the exact arithmetical mean it would have stood at 89·8.*"

Whether the capacities of fluids be permanent from the freezing to the boiling point a problem to be solved.

The affirmative generally believed.

Dr. Crawford's experiment.

I will not here enter into the many difficulties and sources of inaccuracy attendant on this method by mixture, but merely

Remark.

* Crawford on Animal Heat.

merely observe, that when we consider the quantity of caloric unavoidably carried off, the coming so near an exact mean at the end of one minute is very surprising.

Dr. Crawford mistaken, and his data erroneous.

I will now endeavour to demonstrate by direct experiments, that an increase of capacity does invariably take place in a just ratio to the increase of temperature; and in the second place, that a mean, or an approximation to it, may result as well from a gradual increase of capacity, as from a permanency: consequently, that Dr. Crawford's experiments and mathematical propositions are founded on false data.

Experiment which proves the progressive Increase of Capacity.

Experiment to prove this.

The calorimeter being at the precise temperature of 48°, which was also that of the room, I filled the interior compartment, No. 1, with water from a boiling kettle at 212°, and having closed it as before represented with a thermometric cork, I agitated the apparatus well for about the space of $1\frac{1}{4}$ minute, in a horizontal position, when the two thermometers indicated 97°. Therefore the water at 212° had lost 115°, which, being communicated to the water at 48° in the second compartment, had raised its temperature 49 degrees. Having taken down these numbers, I poured out the water from the interior compartment, and brought the calorimeter to the exact temperature of 150°, and again filled the interior compartment with water from the kettle at 212°; when, after brisk agitation as before, I found the temperature to be 166°. Therefore in this experiment the water at 212° had lost 46 degrees, which, being communicated to the water in the second compartment at 150°, raised its temperature but 16 degrees; whereas if equal increments of caloric produced equal increments of temperature, or in other words if the capacity were permanent, it should have raised it $19\frac{69}{115}$, which is easily demonstrated by the following calculation.

If 115 degrees raise water 49 degrees, what should 46 raise it?—Answer, $19\frac{69}{115}$.

Here it is obvious, that the difference between 16 and $19\frac{69}{115}$, is the difference between the capacity of water at 48° and

and the same quantity of water at 150°, in the proportions used in the calorimeter. Or that upwards of 3 degrees became latent, according to Dr. Black; or, what is more simple and philosophical, went to supply the increased capacity.

Having performed this and a number of other experiments at different temperatures, with similar results; and also having repeated them before a most accurate and scientific experimenter, for whose opinions I have the highest respect; and having found them all to coincide, I may justly infer, *that capacities are not permanent from the freezing to the boiling point.* The result confirmed by farther experiments.

I now proceed to show, that a mean, or an approximation to it, may be produced by a gradual increase of capacity. Mean may be produced if the capacity increase regularly.

If I mix water at 100° with water at 50° in equal proportions, a mean of 75° may result. Here 25 degrees with a larger capacity are lost by the water at 100°, which go not only to supply the 25 degrees gained by the water at 50°, but also to fill up that increased capacity, which the water at 50° experienced, to bring its capacity from the freezing point up to an equality of 75°; and we may easily conceive, that they may so nicely balance, as even to produce a mean. Dr. Crawford entirely forgot this increased capacity gained by the water at 50°. This may be more clearly demonstrated by two diagrams, the one representing Dr. Crawford's theory, the other mine.

Suppose *a* and *g* in the parallelogram, Pl. V, fig. 2, to represent the thermometric range, *a b* are equal to *c d*, and *c d* to *e f*, and *e f* to *g h*; therefore, if these are equal to one another, and represent the capacities, the capacities are also equal. This may be all very true; but as similar effects may arise from different causes, I will endeavour to show, how a mean may be produced by a progressive increase of capacity. Dr. Crawford's theory. The proof defective.

Suppose *a g*, fig. 3, to represent the thermometric range from 32° to 100°; No. 4 the capacity of 100°, No. 3 that of 75°, and No. 2 that of 50°, although *a b* is not equal to *c d*, nor *c d* to *e f*, but if we produce from *g h* to *i*, *g i* is equal to *c d*, and if we produce *g i* to *k*, *g k* is equal to *a b*. To demonstrate this in another point of view, if The author's theory.

if we add water at 100°, represented by space 4, to water at 50°, represented by space 2; 25 degrees taken from space 4 of large capacity, not only go to fill up space 3, representing 75°, but also to fill up space 1, the increased capacity gained by 50° in rising from the freezing point.

Although geometrical figures are no evidence of the truth of a chemical doctrine, and should be avoided unless tending to illustrate the subject, yet I thought it necessary to call them to my aid, the more especially as Dr. Crawford has dwelt on them at great length*.

Other fluids obey a similar law.

From these experiments we may analogically infer, that similar laws regulate other fluids in a greater or less degree; and that neither the mercurial, nor any other thermometer, is a faithful index of the quantity of caloric. Thus if the capacity of water increase, it does not bespeak the quantity of caloric thrown in at different temperatures. But, as this is a most important investigation, I will defer the discussion of it to a more voluminous detail; for, should my experiments undergo the ordeal of critical investigation, and be established as facts, the thermometer must be regulated according to the increasing capacity of the fluid, before we can determine the exact quantity of caloric communicated, and there must also be some other method adopted for proving the regularity of mercurial expansion.

The apparatus lost more caloric at a low than at a high temperature.

I will conclude by remarking, that the apparatus abstracted more caloric in rising from 48° to 97°, than from 150° to 166°, and therefore, in that respect, there can be no source of fallacy.

Sir, I beg leave to remain,

Your very obedient servant,

JOSEPH READE, M.D.

Dimensions of the calorimeter used.

P. S. The calorimeter I used held in the interior compartment, No. 1, 6 oz. of water, and in the second 10½ oz. consequently, if equal quantities were used, the increase would be much more.

VII.

* The simple statement of the argument is, that, if the capacities answering to any successive number of degrees of the thermometer be supposed to increase by the augmentation or addition of any constant quantity,

VII.

Experiments on the various Species of Cinchona: by Mr. VAUQUELIN.

(Concluded from page 120.)

Appearances exhibited on a more minute examination by the infusion and decoction of different species of cinchona, that precipitate neither infusion of tan nor tartarised antimony. Barks that precipitate neither tan nor emetic tartar.

THESE sorts of bark impart to cold water a red colour, frequently a yellowish red, sometimes a brown red. Water thus loaded with the soluble part of these barks froths on agitation like wort. Its taste is bitter, and more or less astringent, this differing in the different sorts. Macerated in water.

Left to stand in an open vessel, or in a close one if not full, it soon grows mouldy, and is covered with a greenish pellicle.

Some of them are perceptibly reddened by infusion of litmus, which announces the presence of a free acid. Some of them acid.

Alcohol, mixed with these infusions in the proportion of two parts to one, precipitates a grayish substance, which grows black on desiccation. The fluid is left more clear, and of a purer red. This indicates the presence of mucous matter. Precipitated by alcohol.

In those infusions which have an acid a small quantity of caustic alkali forms a red precipitate inclining to violet: but a large quantity of the reagent redissolves this precipitate, and renders the colour of the infusion more intense. By alkali.

Subjected to evaporation they become higher coloured; and, after being thus boiled down, they let fall on cooling a very bitter brown substance, which dissolves readily in alcohol, particularly with the assistance of heat, and is precipitated from it by water, if the solution be sufficiently saturated Evaporated, form a deposit on cooling.

quantity, the series of capacities will be in arithmetical progression, and the half sum of any two terms equidistant from the same middle (degree or) term will be equal; and the result might therefore be mistaken to indicate an equality of the capacities. N.

rated. Water itself redissolves this substance, though it has been separated from it by evaporation; but it requires a larger quantity, than when it is accompanied with the other principles of cinchona, which seems to show, that these principles promote the solution in water.

This not rendered insoluble by oxigen.

If the infusions of bark be allowed to cool several times, before they are evaporated to dryness, at each cooling they let fall a matter similar to that just mentioned. It was formerly supposed, that this substance was rendered insoluble by combining with oxigen, but the effect appears rather to be owing to the insufficiency of the water.

In this the bitterness resides.

It is this sort of resinous matter, that gives to bark and its infusions their bitter taste: for if these sediments be separated as they form, and the infusion thus boiled down be afterward made up to its former quantity by the addition of water, it will no longer possess the same degree of bitterness. The whole of this matter however cannot thus be separated from water; for the other principles of the cinchona always retain a pretty large quantity in solution.

It is best precipitated by alcohol.

But if, after having proceeded as I have just mentioned, the infusions of cinchona reduced to the state of soft extract be treated with alcohol, the greater part of the *resiniform* matter will be separated; and nothing will remain but a brown viscous substance, that has scarcely any bitterness, is perfectly soluble in water, and does not precipitate from it on cooling.

Two different principles in bark,

These experiments teach us, that in the infusions of these species of cinchona there are at least two very distinct substances: one bitter and astringent, soluble in alcohol, and but little soluble in water; the other on the contrary wholly insoluble in alcohol, very soluble in water, and having a sweet and mucilaginous taste.

in which most of its virtue resides.

These substances being unquestionably those, which operate most efficaciously in the diseases in which cinchona is employed, I conceive it will not be superfluous to give an account of their properties somewhat more at large. I shall begin with that which is soluble in alcohol.

Properties of that which is soluble in alcohol.

1. This substance, in the dry state, has a brown red colour, and a very bitter taste. 2. Cold water dissolves only one portion of it, another

another remaining in a flocculent form and of a reddish colour: but if the mixture be heated, this dissolves too, and the result is a clear liquor, of a very deep red, which grows turbid on cooling, but lets fall very little sediment.

What is remarkable in the manner in which this substance comports itself with water is, that, if we employ but a small quantity of this fluid, it dissolves entirely, and produces a clear liquor: if after this more water be added, it grows turbid; and again it becomes clear on the addition of a still greater quantity of this fluid. Singular effect of water on it.

It would seem from this, that there is some other substance present with it, which promotes its solution when concentrated, and loses this property by being diluted in water. Apparently owing to some other principle.

This is the matter, that renders the decoction or infusion of cinchona turbid, by separating as it cools; as it does the water in which it is macerated, if this be evaporated to a certain point. It is the same as has been called in pharmacy resin of bark: but its solution in water grows mouldy in a few days, and produces fungi, like a solution of gum; which proves it not to be a true resin, for it is well known, that resins never grow mouldy. Erroneously called resin.

The aqueous solution of this substance, recently prepared, and in a somewhat concentrated state, produced the following effects with the different reagents I shall mention. 1. With ammonia it coagulated into a whitish, thick matter, which grew brown in the open air, and hardened considerably a little while after: but it softens by heat, and assumes the ductility and silky lustre of turpentine when kneaded between the hands. Its aqueous solution examined with ammonia,

2. It produced nearly the same appearances with the alkaline carbonates. alkaline carbonates,

3. The common acids produced no sensible change in it. Oxigenized muriatic acid turned it yellow, without producing any precipitation; but if ammonia were then added, a light, flocculent, grayish white precipitate was formed. acids,

4. The solution of animal gelatine does not precipitate it: yet the infusion of these species of cinchona precipitates the solution of animal glue; the principle that produces this effect therefore must be altered during the evaporation. gelatine,

5. The

chalybeates,

5. The muriate of iron, or any other ferruginous salt, produces in it a deep green colour, and soon after a precipitate of the same tint.

emetic tartar,

6. The antimoniated tartrite of potash occasions no precipitation in it. This substance therefore is not the same as that, which in the infusions of certain species of cinchona precipitates this metallic salt.

and litmus.

7. Lastly it very perceptibly reddens infusion of litmus.

Scarcely soluble in water freed from acid;

The acidity of this substance, and the precipitation occasioned by alkalis in its concentrated solution, led me to suspect, that its solubility was in part owing to the presence of the free acid that accompanies it: and this appeared to me to be confirmed by the circumstance, that, when once separated by an alkali, washed, and dried, it was no longer soluble in water but in an infinitely small proportion.

unless an acid be added to the water.

To acquire a greater degree of certainty upon this subject, I put some into water acidulated with various acids; and I found in fact, that it dissolved in them readily, and that its solutions resumed a bitter taste, similar to that it had before it was precipitated by an alkali.

Seems to retain some of the alkali that threw it down.

I remarked, that this substance, when precipitated, retained a part of the alkali employed to throw it down: at least the following experiment seemed to prove this. After its solution had been precipitated by ammonia, and washed in a large quantity of water, I mixed with it caustic potash, which immediately produced a very evident smell of ammonia; and this was not the case, before it had been precipitated by that alkali.

It is evident therefore, that this substance combines with a portion of the ammonia, which is employed to precipitate it from its solution; unless the acid, which naturally accompanies it, forms with this alkali an insoluble salt, that mixes with the resinous matter, a circumstance that appears not very probable.

Neutralizes both acids and alkalis.

It seems from these properties, that this substance acts the part sometimes of an acid, at others of an alkali, since it combines with both these, and in part neutralizes their properties.

Soluble in excess of alkali.

If, after having precipitated this matter by alkalis, an excess of

of these reagents be added, it is redissolved, and the solution has a brown red colour.

The solubility of this substance in alcohol is singularly increased by heat. When the menstruum is saturated with it, it has a red colour, and an extremely bitter taste. Water throws down from it a copious precipitate of a fine red slightly inclining to rose-colour. The alcoholic solution, exposed to the air in an open vessel, crystallizes in a needly form like a salt. Heat greatly increases its solubility in alcohol.

The alcoholic solution precipitated by water still retains a portion of this substance, which continues to give it a rose-colour inclining to deep orange [*nacarat*], and a perceptibly bitter taste. It deposits this in scales of a brown red by spontaneous evaporation. Tincture precipitated by water.

That principle of the cinchona, which is insoluble in alcohol, being dissolved in water, filtered, and left to spontaneous evaporation in a warm place, thickens like a kind of sirup, and crystallizes in laminæ, sometimes hexaedral, at others rhomboidal, at others square, and slightly tinged with a reddish brown. A portion of a thick fluid always remains, which never crystallizes completely, and which must be separated by decantation. Principle insoluble in alcohol. Yields a salt.

By repeated solution and crystallization this salt may be obtained white and pure. Of its properties I shall speak hereafter. As to the matter that does not crystallize, but remains in the form of a mother water, it exhibited all the characters of a mucilaginous matter, still retaining a small portion of the salt I have just mentioned, which it is impossible to separate from it entirely by crystallization. Which may be purified. The remainder mucilaginous.

Action of acids on the residuums of cinchona exhausted by water.

The barks in question, after being exhausted by water, and even by alcohol, still yield something to acids. They all act nearly in the same manner: that is to say, their effect is confined to simple solution, without occasioning any perceptible change in the nature of the principles of the cinchona. Action of acids after water.

I must observe however, that, if the bark have been reduced to fine powder, and subjected to the repeated action of a large Dissolve the part soluble in alcohol.

large quantity of alcohol assisted by heat, little is left to be done by the acids. The matter taken from the bark by acids is acccording to all appearance the same, as that which dissolves in alcohol, as I shall show farther on.

Nitric acid.

Nitric acid acquires from it a red, inclining to rose-colour, and sometimes to a deep orange [*nacarat*]: but these tints vary greatly in their intensity according to the strength of the acid; the stronger this is, the more they incline to yellow. The nitric acid loses much of its acidity by this combination, at least as far as we can judge from the taste: it is true it dissolves at the same time a certain quantity of lime, which is detected by oxalate of ammonia, and this contributes to its neutralization.

Action of carbonates on the solution:

If saturated carbonate of potash be poured into this nitric solution, a fine red precipitate is formed: but if the common carbonate be employed, and added in excess, the colour of the precipitate becomes violet, purple, or blue. Thus alkalis have the property of blueing that colour of these barks, which is naturally red.

and of solutions of metals.

Metallic solutions likewise form in it precipitates of various colours, and more or less abundant, according as the nitric acid contains more or less vegetable matter: but, if the excess of acid be saturated, the metallic salts then produce in it very copious precipitates, and the liquor is deprived of colour.

1. Solution of muriate of tin produces in it a rose-coloured or carnation precipitate.

2. That of sulphate of iron, a grayish precipitate.

3. That of copper a chesnut brown.

4. Sulphate of titanium, assisted with a little carbonate of soda, formed with the nitric solution of cinchona an orange red precipitate, pretty analogous in colour to that produced by solutions of this metal with galls.

5. Alum occasioned no change in the acid solution of cinchona: but aided by a little alkali it carries down with it the colouring part, and the liquor is rendered colourless.

Might be employed as a dye.

In the countries where these cinchonas grow, a very fine and permanent chesnut red for wool and cotton might be obtained from their bark. Soap turns it to a rose colour.

Sulphuric and muriatic acids.

The sulphuric and muriatic acids, diluted with water, and poured

poured on the residuums of these cinchonas, dissolve the resiniform substance, and saturate themselves with it like the nitric acid. The colour they thus acquire inclines less to yellow than that of the nitric acid: it is always of a more decided red.

The precipitates formed in these solutions by alkaline carbonates are likewise of a purer red; and an excess of these alkaline salts gives the precipitate a more evident blue. Action of carbonates on these solutions.

The residuums of the cinchonas appear to contain a large quantity of lime: at least a great deal of sulphate of lime is produced by spontaneous evaporation in the sulphuric acid in which they have been macerated. Lime in the residuums.

From the action of acids on the resiniform matter of these species of cinchona, if it should at any future time be demonstrated, that this substance is the only febrifuge principle in them, it is evident, that the art of physic may derive from these barks much more advantage in the cure of intermittent and low fevers, by adding to them acids or wine. In fact, as has been seen above, water extracts from cinchona, particularly when it is merely bruised, but a very small quantity of resiniform matter, and even the greater part of this is precipitated by cooling. Now by this means it is certain, that from a large quantity of cinchona we extract but a very small part of the febrifuge principle*; which too, being diffused through a large body of water, unquestionably cannot produce all the effect, of which it would be capable in a more concentrated state. Remarks on the action of acids.

It has long been known, that the effect of the essential salt of cinchona in fever is by no means proportional to that of the quantity of bark from which it has been extracted: which proves, that something useful in the cure of this disease is left in the magma. Its essential salt.

According to my way of thinking, the method hitherto pursued for preparing the essential salt of bark is the reverse of what it ought to be. When an infusion of cinchona is made, Hitherto extracted by a bad process.

* Mr. Vauquelin has forgotten his *if*, in the first sentence of this paragraph. It has not yet been proved, that this is the febrifuge principle: and indeed he himself had before ranked the principle soluble in water with it in this respect. Tr.

it is evaporated to a certain point, left to grow cool that it may deposit a sediment, this resiniform sediment is separated from the liquor, and the evaporation and refrigeration are repeated, till the liquor no longer becomes turbid, and has only a pale yellow colour. It is then dried on plates by the heat of a stove. By operating thus a very small quantity of resiniform matter only remains in the water, with a gum, and a salt with a calcareous basis, the efficacy of which in the cure of fever is very questionable.

Comparative examination of the resin of these cinchonas with other known vegetable substances.

Is the matter soluble in alcohol a resin?

Is there in the vegetable kingdom any immediate principle, with which this can be classed? Is it to be placed among the resins, as has hitherto been done? It is true that chemists and apothecaries formerly arranged together so many substances under this genus, that, if we looked to some of its properties only, we might also rank this among them: but if we apply the name of resin only to those substances, which are absolutely entitled to it, those of cinchona and many other vegetables must be separated from the resins properly so called.

No:

If the resiniform matter of these cinchonas resemble resins by its solubility in alcohol, it differs from them by its solubility in water, acids, and alkalis, and particularly by its property of precipitating metallic salts, and fixing in cloth. I believe then it may be considered as a peculiar vegetable principle, the properties of which have not hitherto been well understood by chemists. This principle is not the same in every species of cinchona: it differs in those that precipitate infusion of tan and tartarised antimony, and in those that precipitate isinglass only.

but a peculiar principle.

Perhaps similar to what gives bitterness.

It is probably a principle extremely analogus to it, that most commonly imparts a bitter taste to vegetables.

Recapitulation of the properties of cinchonas.

General properties of barks.

1. The different species of bark may be divided into three classes with respect to their chemical properties.

In

In the first may be comprised those, that precipitate tannin, and do not precipitate animal glue. Three classes of them.

In the second, those that precipitate animal glue, and do not precipitate tannin.

In the third, those that precipitate both tannin and animal glue, and also tartarised antimony.

2. We may conjecture with sufficient probability, that every vegetable substance, which does not possess at least one of the properties above mentioned, will not be a febrifuge; and it is probable too, that the more these properties unite in a cinchona, or in any other substance, the more striking will be its febrifuge effects. Indication of febrifuge properties.

3. The property of precipitating tannin not being common to all the cinchonas, it is not from this exclusively, that they derive their febrifuge virtue; for there are several that do not precipitate it, and yet cure intermittent fevers.

4. It appears however, that the principle which precipitates infusion of oak bark and nutgalls is febrifuge, for the species that produce this effect are generally allowed to be the best for medicinal use.

5. On the other hand, since cinchonas which precipitate neither infusion of tan nor nutgalls are febrifuge, we must conclude, that the principle, by which these precipitations are produced, is not the only one in cinchona, that cures fever. These not confined to one principle.

6. The principle that precipitates infusion of tan and nutgalls has a brown colour, and a bitter taste; it is less soluble in water than in alcohol; it precipitates likewise tartarised antimony, but not isinglass. It has some analogy with resinous substances, though it affords ammonia by distillation. Principle that precipitates tan.

7. It is apparently with the tannin of oak bark and nutgalls, that this principle combines to form the precipitates it occasions in the infusions of these substances: yet, as this principle exists in some species of cinchona, that precipitate isinglass at the same time, it remains questionable, whether it actually combine with the tannin of the infusion of oak bark, or whether the principle, that in other species of cinchona precipitates isinglass, be real tannin. Doubts.

 8. But

8. But one or the other of these suppositions must necessarily be true, since the infusions of these two sorts of cinchona mutually precipitate each other.

Principle that precipitates gelatine.

9. The principle, which in some species of cinchona precipitates isinglass, has a bitter and astringent taste: it is more soluble in water than that, which in other species precipitates infusion of tan: it is likewise soluble in alcohol: and it does not precipitate tartarised antimony.

10. It appears, that the substance which precipitates infusion of tan is the same, as that which decomposes antimoniated tartrite of potash.

Knowlege of the febrifuge principle a desideratum.

We see from all these doubts, that much remains yet to be done, before we shall attain an accurate knowledge of the principle or principles in cinchona, from which it derives its efficacy in the cure of fevers. It is to be hoped, that time and assiduity will accomplish the solution of this important question.

Analysis of the salt of cinchona.

Salt of cinchona.

Mr. Deschamps, jun., a druggist at Lyons, is the first to my knowledge, who announced the presence of a peculiar salt in cinchona, which must not be confounded with the essential salt of la Garaye, for this contains at the same time both resin and mucilage: but as Mr. Deschamps has described only some of the physical properties of this salt, I thought it necessary to analyse it, in order to discover the nature and proportions of its principles. I have already said how this salt may be obtained and purified: here therefore I shall confine myself to an account of its properties.

Its characters.

1. This salt is white; crystallizes in square laminæ, which are sometimes rhomboidal, or truncated at their solid angles; and these laminæ frequently unite in clusters.

2. It has scarcely any taste, and is flexible between the teeth.

3. It requires about five parts of water at 10° [50° F.] for its solution.

4. On burning coals it swells up like tartar, and emits a similar smell; and leaves a grayish substance, which dissolves in

in acids with effervescence, and is nothing but a mixture of carbonate of lime and charcoal.

5. Its solution does not alter the colour of litmus. In alcohol it is completely insoluble.

6. The fixed alkalis, whether caustic or carbonated, decompose it, and precipitate lime from it, either pure or in the state of carbonate.

7. It is not decomposed by ammonia; which proves, that its acid has a stronger affinity for lime.

8. Both sulphuric and oxalic acids form a precipitate in its solution, if it be in a tolerably concentrated state; the result being sulphate or oxalate of lime.

9. It produces no apparent alteration in solution of acetate of lead, or of nitrate of silver.

10. Concentrated sulphuric acid, poured on this salt reduced to powder, blackens it slightly; but it does not emit any of the pungent vapour evolved from acetates.

11. A remarkable circumstance is, that the infusion of tan, and of some species of cinchona, that of Santa Fe for instance, occasions a yellow flocculent precipitate in the solution of this salt.

A compound of lime and some acid.

The various phenomena produced by these experiments indicating, that this salt consisted of a vegetable acid and lime, in order to decompose it, and obtain the acid separate, I employed oxalic acid, which is well known to render lime most insoluble by combining with it. With this view I proceeded in the following manner.

Analysis of it.

I dissolved 100 parts of this salt in as much water as was requisite. Into this solution I poured a solution of oxalic acid, from a quantity of a known weight, at different times, till no precipitate was formed. About twenty-two parts were necessary, to precipitate the whole of the lime, yet I obtained but twenty-seven parts of dry precipitate.

This proves, that the oxalic acid employed retained about half its weight of water of crystallization; and that the salt of cinchona contained but a small quantity of lime, for in twenty-seven parts of oxalate of lime there are but fifteen of this earth at most.

After having thus separated the lime from this salt by means of oxalic acid, I allowed the supernatant liquor to evaporate

evaporate spontaneously; and it was thus reduced to the state of a very thick sirup, without affording any sign of crystallization, after it had stood above a week. Having stirred it however with a piece of glass, in order to take out a portion which I intended for another experiment, I was surprised to find the fluid crystallized a few instants after into a hard mass, formed of a great quantity of laminæ, diverging from several very distinct centres of crystallization.

The acid crystallized suddenly on agitation.

It was slightly tinged of a brown colour: its taste was extremely acid and a little bitter, because the salt of cinchona I had employed had not been perfectly purified.

I shall now proceed to the properties I observed in this acid, on which however I cannot enlarge very minutely, as I had but a moderate quantity of the salt at my disposal. I believe however, that I have examined it sufficiently, to be convinced of its being a peculiar acid hitherto unknown.

Its properties.

In its state of crystallization it has a very acid taste, and is a little bitter*, as I have said above.

It keeps perfectly well in the open air, being neither deliquescent nor efflorescent.

On burning coals it melts very quickly, boils, grows black, emits pungent white vapours, and leaves but a very light coally residuum.

With the earths and alkalis it forms soluble and crystallizable salts.

It does not precipitate nitrate of silver, mercury, or lead, as most other vegetable acids do.

It is a new vegetable acid, differing from

There appears no doubt, that this acid is new to us; for, on reviewing the characters of all the other vegetable acids known, neither of them unites in it all the properties of this.

the oxalic,

In fact oxalic acid forms an insoluble salt with lime, and besides decomposes the compound formed of this earth and acid of cinchona.

citric, and tartarous,

The citric and tartarous acids form likewise insoluble salts with lime, and decompose acetate of lead.

malic,

The malic acid does not crystallize, and precipitates acetate of lead.

* Mr. Vauquelin has just been ascribing this bitterness to the impurity of the salt he employed, consequently it is not a character of the acid. Tr.

The

The benzoic acid is but little soluble in cold water, and is volatilized without being decomposed. benzoic,

The gallic acid too is but little soluble in cold water, and blackens solution of iron. gallic,

It is analogous to the acetous acid in the solubility of its combinations; but the acetous acid does not crystallize, and is volatilized without alteration. acetous,

I say nothing of the camphoric, suberic, or succinic acids, for they bear no analogy to it. and others.

Let us then conclude, that this acid is really different from all those hitherto known, and give it the name of *kinic* acid, from the word *quinquina*, till, becoming more ultimately acquainted with its nature and combination, we can frame a better. The *kinic* acid

It is to this acid united with lime, according to the report of Mr. Deschamps, that the physicians of Lyons ascribe the febrifuge virtue of cinchona. They assert, that no intermittent fever can resist two doses of this salt of thirty-six grains each. combined with lime said to be the febrifuge substance.

If this assertion were proved, we might pretty easily conceive how a drachm of this salt cures an intermittent fever, for this quantity is as much as can be obtained from at least five or six ounces of common gray bark.

I cannot directly contradict this result, announced by persons of credibility and well informed; yet I think I have sufficient reason, to entertain some doubts of its accuracy. In the first place, before it can deserve complete confidence, it must have been tried a great number of times, and with uniform success: for it too often happens, that effects are ascribed to medicines, which in fact are owing entirely to nature. In the art of physic, more than in any other branch of natural philosophy, causes are so complicated, that it is difficult to trace with certainty what belongs properly to each. This questionable, for different reasons.

On the other hand physicians have learned by long experience, that the infusions and extract of bark prepared after the manner of la Garaye are far from producing equal effects in fever with the quantity of bark from which they are prepared, if this were administered in its natural state: yet these preparations contain the salt in question.

It is known too, that spirituous tinctures of bark, in which

in which the salt of Mr. Deschamps does not exist, since it is insoluble in such menstruums, cure intermittent fevers.

Besides, there are cinchonas, which contain but extremely small quantities of this salt, and vegetables in which none of it is found, that likewise cure fevers. It is not then without reason, as is obvious, that I express my doubts on this head: and if it have sometimes happened, that this salt has cured fever, we may suspect, that it had not been perfectly freed from the bitter principle, which it strongly retains.

Desirable that it should be tried.

It is desirable however, that this question should be resolved by experiment as soon as possible: for, if the results of experience be conformable to those of the physicians of Lyons, it would certainly be a very useful discovery for mankind.

VIII.

On the Quantity of Carbon in Carbonic Acid, and on the Nature of the Diamond. By William Allen, *Esq. F. L. S. and* William Hasledine Pepys, *Esq. Communicated by* Humphry Davy, *Esq., Secretary R. S. M. R. I. A.**

Quantity of carbon in carbonic acid not ascertained, and experiments on the diamond objectionable.

THE estimates of the quantity of real carbon in carbonic acid differing very widely, and the experiments of Guyton de Morveau upon the combustion of the diamond, detailed in the 31st volume of the *Annales de Chimie*, being liable to some objections, from the manner in which the operations were conducted; we determined to institute a set of experiments, in order, if possible, to settle the question.

Lavoisier, from the result of experiments apparently conducted with much accuracy, concluded, that every hundred parts by weight of carbonic acid consisted of 28 carbon and 72 oxigen. This was in a great degree confirmed by the

* Philos. Trans. for 1807, Part II, p. 267.

very

very valuable researches of Smithson Tennant, Esq., on the nature of the diamond, an account of which is printed in the Transactions of this Society for the year 1797, and which were made previously to the experiments of Guyton; but notwithstanding this, the result of Guyton's experiment, which only allowed 17·88 per cent of carbon to carbonic acid, has been adopted in all the systems of chemistry to the present time.

In researches of this nature, the results are much influenced by slight variations in the quality of the gas; but having had repeated experience of the accuracy of the eudiometer described in No. XII, of this volume*, we were enabled to proceed in this respect with great confidence.

Attempt to ascertain the facts.

Our object was, to consume certain known quantities of diamond and other carbonaceous substances in oxigen gas, and we at first determined to employ the sun's rays, by means of a powerful lens; but considering the uncertainty of a favourable opportunity in this country, and at the season in which our experiments were made, we resolved to employ the apparatus respresented by the drawing.

Description of the apparatus.

Apparatus described.

This consisted of two mercurial gasometers, Pl. VI, fig. 1 and 2, each capable of containing from 70 to 80 cubic inches of gas. The internal cylinder C C is of cast iron, and solid, except the perforation through its middle; the external cylinder is also of cast iron; and the glass receiver slides up and down in the space between them, which is filled with mercury: not more than 16 pounds are required for each, and the small bath B, fig. 1.

To the top of each receiver a graduated scale or register, H, is screwed, showing the number of cubic inches of gas, measuring from the upper edge of the external iron cylinder. The level of the mercury is ascertained by a small glass gauge. The registers were graduated by throwing up one cubic inch of gas at a time.

The gasometers stand upon mahogany stools, perforated for a socket, to which, according to the nature of the expe-

* See our last Number, page 86.

riment,

riment, a small receiver R, or the triple socket T S, or any other combination, may be united.

P represents the platina tube with its furnace; the ends of the tube are mounted with female screws of brass, to one of which the accommodating screw socket A S was joined.

T is a double section of the platina tray, which contained the substances to be heated. During their combustion, it was made to slide easily within the platina tube P. The accommodating socket and platina tray are drawn considerably larger in proportion than the instrument.

By means of the triple socket and the cocks, the gas was made to pass freely over the substances in combustion, from one gasometer to the other; and by shutting off the communication with the platina tube, while that with the small receiver was open, any portion of gas in the gasometer, fig. 1, might be transferred into eudiometers or measures standing in the mercury bath M, for examination.

In order to discover whether the several sockets were air-tight, after the apparatus was put together, the communication with the gasometer, fig. 1, was closed, and the other communications opened; the receiver of the gasometer, fig. 2, being raised, drew up a column of mercury in the small receiver R, equal to 2 inches: the communication with the gasometer was then closed, and the column was supported without alteration. This was always tried previous to, and after every experiment. As the joints would bear this degree of exhaustion, we were confident they would resist a much greater pressure than we had any occasion to employ. The glass tubes G G, which connected the platina tube with the gasometers, enabled us to observe any flash arising from the combustion of hidrogen which might be contained in the substances subjected to experiment. In order to avoid prolixity, we shall generally state the method which was invariably followed.

Oxigen gas injured by keeping.

We soon found that oxigen gas, even when secured in bottles with ground glass stoppers, was not always to be depended upon, but was sensibly deteriorated by keeping; and therefore in all our experiments we made the gas within an hour or two of the time of using it, and always from the hyperoxigenised muriate of potash. Its degree of purity was constantly

Manner in

constantly ascertained by the eudiometer before every experiment, and was generally determined in about 10 minutes. The solution employed was that recommended by Professor Davy; namely, the solution of green sulphate of iron saturated with nitrous gas*; and whenever the diminution had arrived at its maximum, and the gas began to increase in volume, we substituted a simple solution of the green sulphate of iron for that saturated with nitrous gas, and always had the most satisfactory results: for the simple sulphate absorbs any nitrous gas which may have escaped from the saturated solution, and the residuum in this case enables us to ascertain exactly the quantity of oxigen contained in the gas. which its purity was ascertained.

We determined to make our first experiment with charcoal, and as Morozzo and Rouppe had ascertained the absorbing properties of this substance, and as our results must obviously be influenced by it, our attention was directed to this point. The following quantities of different kinds of wood, sawed into slips $\frac{1}{10}$ of an inch were weighed. Woods charred.

Their weight.

White Fir········	300 grains
Lignum Vitæ ····	800
Box ············	400
Beech ··········	500
English Oak······	250
Mahogany ······	200

These slips were put into small crucibles, and completely covered with dry sand. Heat was very gradually applied at first, until the volatile parts were dissipated; they were then kept about 40 minutes in a white heat. On being collected and weighed, while still warm, the charcoal from each was as follows: In small crucibles under dry sand.

Weight of charcoal produced.

Fir ········	54·5	grs. equal to	18·17 per cent.
Lignum Vitæ	138	··········	17·25
Box········	81	··········	20·25
Beech······	75	··········	15
Oak ······	43·5	··········	17·40
Mahogany ··	31·5	··········	15·75

* This solution absorbs oxigen much more rapidly in warm weather than in cold.

These

Gain by a week's exposure to air.

These being exposed to the air during one week, increased in weight thus:

Fir	13 per cent.
Lignum Vitæ ..	9·6
Box	14
Beech	16·3
Oak	16·5
Mahogany	18

This probably by absorption of water.

Certain quantities being *confined* in common air increased very little in weight, and all in the same proportion; we are therefore much inclined to think, that *this* increase is owing to an absorption of water from the air; and we repeatedly found, that the greatest increase of weight took place in the first hour or two after exposure, and arrived at its maximum in less than 24 hours, as the following experiment, selected from several others, will prove.

Experiment with willow charcoal.

Forty grains of charcoal from willow wood, which had been put into a bottle with a ground glass stopper *immediately* after they were removed from the fire, were exposed in the scale of a delicate balance, in a room where the thermometer was 62° Fahrenheit, barometer 30·26.

	Grains.		Total Increase.	Time.
6 o'clock P. M.	40			
½ past	40·7	+ ·7		
7	41·3	+ ·6	= 1·3	1 hour.
½ past	41·6	+ ·3	= 1·6	1½ hour.
8	41·8	+ ·2	= 1·8	2 hours.

The pieces were now spread out on paper after every weighing, to expose them more completely.

	Grains.		Total Increase.	Time.
½ past 8	42·5	+ ·7	= 2·5	2½ hours.
9	42·8	+ ·3	= 2·8	3 hours.
½ past	43.1	+ ·3	= 3·1	3½ hours.
10	43·3	+ ·2	= 3·3	4 hours.
½ past	43·4	+ ·1	= 3·4	4½ hours.

Here it was left all night.

	Grains.		Total Increase.	Time.
10 A. M.	45	+1·6	= 5	16 hours.
4 P. M.	45			

6 P. M.

	Grains.		Total increase.	Time.
6 P.M.	44·5 — ·5	=	4·5	24 hours.
9	44·4 — ·1	=	4·4	27 hours.
	Next day.			
$\frac{1}{2}$ past 8 A.M.	44·9 + ·5	=	4·9	38$\frac{1}{2}$ hours.
$\frac{1}{2}$ past 1 P.M.	44·7 — ·2	=	4·7	43$\frac{1}{2}$ hours.
10	44·5 — ·2	=	4·5	52 hours.

Hence charcoal seems to act as an hygrometer: its greatest increase was 5 grains on 40, or 12$\frac{1}{2}$ per cent. And in order to ascertain to what the increase of weight was owing, we put 27·25 grains of charcoal, which had been thus exposed, into a small bottle and tube connected with a receiver standing in the mercury bath, the whole of the vessels being also filled with mercury, in order to exclude common air. Heat applied by an Argand's lamp produced gas equal to about *half the bulk of the charcoal;* but as soon as the temperature of the mercury rose to 214° Fahrenheit, elastic fluid streamed from every piece of charcoal, which *quickly condensed,* and 1$\frac{1}{2}$ inch of the tube was occupied with water. This proved that our suspicion of the increase of weight being principally attributable to water, was well founded.

Seems to act as an hygrometer.

Water expelled from it.

The result of these, and other experiments, plainly pointed out the precautions which were necessary, in order to obtain an accurate result with charcoal; for if we had weighed 4 grains of the charcoal a few hours after it was made, we should only in fact have had 3·5 grains of real charcoal, and our calculations would have been erroneous. To avoid this source of errour, we subjected our charcoal to a red heat *immediately* before using it, and also weighed it as speedily as possible; in fact, while it was still warm. It may be proper to state, that our weights were such as we could thoroughly depend upon.

Hence certain precautions necessary.

The volume of gas being so much influenced by temperature and pressure, these were noted during every experiment; and thermometer 60° Fahrenheit, barometer 30°, were assumed as the standard. Gay Lussac remarks, that from 32° to 212° Fahrenheit, dry air expands 0·00208, or $\frac{1}{480}$ part of its bulk for every degree of the thermometer. Dalton makes it 0·00207, or $\frac{1}{483}$ part; we therefore divided the

The volume of gas much influenced by temperature and pressure.

the whole quantity of gas by 480, and multiplied the quotient by the degrees of difference under 60°.

Sp. grav. of oxigen and carbonic acid gasses determined.

It being of great consequence in these experiments to know the *exact* weight of a given quantity of oxigen and carbonic acid gasses, we resolved to examine for ourselves, whether the statements already given were quite correct, and accordingly made carbonic acid over mercury from Carrara marble and diluted sulphuric acid, which, being tried with lime water in Pepys's eudiometer, was all absorbed in 3 minutes, except 1 part in 100. We used two charges of lime water, though one would have been sufficient.

Carbonic acid gas.

A glass globe, being exhausted by an excellent airpump, was exactly balanced on a beam sensible to a minute portion of a grain: then being screwed upon one of the glass receivers of the mercurial gasometer previously filled with carbonic acid gas, 21 cubic inches entered. The globe was now increased in weight by 10·3 grains, In order to be certain, we repeated the experiment, with exactly the same results. The 21 cubic inches were to be brought to the mean temperature and pressure, as the thermometer stood at 44° Fahrenheit, the barometer 29·86.

21	480)21·00(0·043	60°
·68 add for temp.	16	44
21·68	0·688 add for temp.	16 diff.

Correction for pressure.

30 : 29·86 : : 21·68 : 21·58

The volume therefore at mean temperature and pressure would have been 21·58 cubic inches.

21·58 : 10·2 : : 100 : 47·26

100 cub. inches weigh 47·26 grs.

Consequently 100 cubic inches of carbonic acid gas at mean temperature and pressure weigh 47·26 grains.

Oxigen gas.

We next tried oxigen gas from the hyperoxigenised muriate of potash made over mercury, and which by the eudiometer left only a residuum of 2 parts in 100. The glass globe, exhausted as before, and weighed, was screwed on to the glass receiver of the mercurial gasometer containing oxigen, and 21 cubic inches entered, by which it increased in

in weight 7·3 grains. This experiment was repeated with exactly the same result. The thermometer and barometer remaining the same, we take the volume as before corrected.

21·58 cubic inches.

21·58 : 7·3 :: 100 : 33·82

Then 100 cubic inches of oxigen gas at mean temperature and pressure weigh 33·82 grains. After these experiments, we examined Davy's researches on nitrous oxide, and had the satisfaction to find, that his estimate, both of carbonic acid and oxigen gasses, agreed almost exactly with ours. 100 cub.inches weigh 33·82 grs.

The next point was to ascertain whether limewater would take the whole of the carbonic acid gas from a mixture with oxigen, or common air; we therefore mixed a known quantity of carbonic acid gas with a certain quantity of common air, and on trying it with our eudiometer and limewater, the *whole* of the carbonic acid gas was in a short time absorbed. We also found, that, though the solution of green sulphate saturated with nitrous gas would not take up the whole of the carbonic acid gas, yet the simple green sulphate, merely by its water of solution, absorbed it very readily. Whole of carbonic acid gas absorbed from common air by limewater or green sulphate of iron,

It may be proper to notice here, that though we repeatedly tried the oxigen procured from hyperoxigenised muriate of potash by the eudiometer and limewater, it never gave the least trace of carbonic acid. Gas from hyp. mur. of potash contained no trace of carbonic acid.

Experiment with Charcoal from Box-wood.

The thermometer being at 42° Fahrenheit, barometer at 30·2, we kept some box-wood charcoal red hot for a considerable time under sand, and weighed 4 grains as expeditiously as possible; this, being put into the platina tray, was pushed to the middle of the platina tube; the oxigen (made from hyperoxigenised muriate of potash over mercury) was contained in gasometer No. 1; No. 2 was empty. Every thing being adjusted and found perfectly air-tight, the communication with the small receiver R was closed, and the common air contained in the tubes and sockets, amounting only to 2·84 cubic inches, was driven out by a pressure of oxigen from gasometer No. 1. When several cubic inches had Experiment with box wood charcoal.

had passed into gasometer No. 2, the gas was let out by opening the cock at the top of its glass receiver, and pressing it down; the cock being then closed, the gasometer No. 2 was completely empty, and the whole of the gas from No. 1 was driven through the tubes into No. 2, and back again. The common air having been previously withdrawn from the small receiver B, we tried the purity of our oxigen by the eudiometer in the manner before described, and found a residuum of 3 parts in 100: we then disengaged as much gas as reduced the quantity to 47 cubic inches by the register or scale; to this must be added the contents of the tubes and sockets 2·84 cubic inches, making the total quantity of oxigen employed 49·84 cubic inches.

Correction for temperature.

49·84	480)49·84(0·103	60°
1·85 for temp.	18	42
51·69	1·854 add for temp.	18 diff.

Correction for pressure.

30 : 30·2 : : 51·69 : 52·03.

The volume, therefore, at mean pressure and temperature, would have been 52·03 cubic inches.

Burned in the platina tube with oxigen gas.

We now lighted a fire in the small black lead furnace under the platina tube, and, as soon as it became red hot, opened the cocks, and passed the gas from No. 1 to No. 2, when the charcoal entered into vivid combustion, and heated the platina tube white hot. The operation was repeated many times during 6 or 7 minutes, by pressing alternately upon the glasses of the gasometers. Not the least flash of light was observable in the glass connecting tubes G G, nor the smallest appearance of moisture. The furnace being removed, the tube was now cooled by the application of wet cloths; and when all was reduced to the temperature of the room, we pressed upon the glass of gasometer No. 2, so as to force all the gas into No. 1. The cock below being closed, we tried the tubes, &c. and found them perfectly air-tight. We next unscrewed the tube and took out the platina tray; but it only contained a light white ash, somewhat resembling the

No flash of light or appearance of moisture.

the shape of the pieces of charcoal, and weighing only ·02 of a grain. On observing the register of No. 1, it indicated exactly the quantity of gas that we began with, so that although 3·98 grains of charcoal had been dissolved, the volume of gas was *unaltered* by it; a circumstance which had been remarked before by Lavoisier. The small receiver R was now nearly full of mercury; the communication with the gasometer being opened, the large glass receiver was gently pressed upon, until several cubic inches were forced through the receiver R, and tube K, in order to clear the latter of common air. This being done, on trying our gas with the eudiometer and limewater, 56 parts were absorbed out of 100. These of course were carbonic acid gas; the test for oxigen absorbed 41, and a residuum of 3 was left, which was exactly what we began with. This is a striking proof, that nothing but carbonic acid was produced in the experiment.

Left ·02 gr. of white ashes.

The volume of gas unaltered.

Nothing produced but carbonic acid gas

100 : 56 : : 52·03 : 29·13.

Then 29·13 cubic inches of carbonic acid gas were produced.

29·13 cub. inches,

100 : 47·26 : : 29·13 : 13·76.

These 29·13 cubic inches of carbonic acid gas would therefore weigh 13·76 grains.

weighing 13·76 grs.

The charcoal weighed	4	grains.
The residual white ash	0·02	
Charcoal consumed ··	3·98	grains.

Then if 13·76 grains, the weight of the carbonic acid produced, contain 3·98 of charcoal, 100 grains must contain 28·92.

13·76 : 3·98 : : 100 : 28·92.

Then, according to this experiment, 100 grains of carbonic acid gas contain 28·92 charcoal.

The gas before the experiment consisted of

Oxigen	50·47	cubic inches.
Azote··	1·56	
	52·03	

After the experiment,

Carbonic acid	29·13	cubic inches.
Oxigen	21·34	
Azote	1·56	
	52·03	

Now as the volume of gas was unaltered, it will be fair to consider the quantity of oxigen gas consumed as equal to the carbonic acid produced, or 29·13 cubic inches.

100 parts of carbonic acid gas by weight contain 71·23 oxigen gas, 28·77 charcoal.

Then, if 100 cubic inches of oxigen weigh 33·82 grains, 29·13 cubic inches will weigh 9·85 grains.

100 : 33·82 : : 29·13 : 9·85.

The weight of oxigen consumed was therefore	9·85	grains.
Charcoal consumed	3·98	
Carbonic acid from this statement ..	13.83	grains.
Ditto by calculations on carb. acid gas	13·76	
	·07	

13·83 : 3·98 : ; 100 : 28·77.

Thus, calculating by the oxigen consumed, 100 grains of carbonic acid gas contain 28·77 charcoal,

First Experiment on Diamond.

Exp. 1. On diamond.

Thermometer 56° Fahrenheit, barometer 30·20.

Our oxigen was made as in the former experiment; it contained no carbonic acid; and, on being tried with the impregnated green sulphate, left a residuum of 3 parts in 100.

3·95 grs Brazil diamond burned as the charcoal.

Having selected nine of the clearest and most transparent Brazil diamonds, we found they weighed 3·95 grains. These were ranged in the platina tray, which was placed in the tube, and the whole apparatus, adjusted as before, was perfectly airtight. The quantity of oxigen was 49·84 cubic inches, as in the last experiment. The same precautions were used to secure accuracy in the results as in the former experiment; and it would only be an unnecessary intrusion on the time of the Society to repeat them. The platina tube was heated red hot, and kept so for ten minutes: during this time the gas was repeatedly

peatedly passed from one gasometer to the other; the tube did not become white hot, as in the experiment with charcoal, because in this case the combustion went on more slowly. When every thing was cooled to the temperature of the room, the gas was all passed into No. 1, by pressing down the receiver of No. 2, and the volume was precisely the same as when we began the experiment. On drawing out the tray, we observed that some of the diamonds were reduced to a minute speck, and all of them resembled opake white enamel: there was no discoloration in the tray, nor any residual ash whatever; the unconsumed parts weighed 1·46 grains; the original weight

The combustion less vivid.

Residuum an opake white enamel.

was 3·95
1·46

consequently 2·49 grains were consumed.

2·49 grs. consumed.

We could not perceive any dullness on the surface of the mercury in the gasometers, or any appearance of moisture.

No moisture appeared.

On introducing limewater to 100 parts of the gas in the eudiometer, a dense white precipitate was formed, and 36 parts absorbed; the test for oxigen absorbed 60, and a residuum of 4 was left.

Residual gas increased ·01.

Correction for temperature.

60°	480)49·84(0·103	49·84	
56	4	·41	add for temp.
4 difference.	·412	50·25	

Correction for pressure.

30 : 30·20 : : 50·25 : 50·58.

The quantity of oxigen at the mean was 50·58 cubic inches.

100 : 36 : : 50·58 : 18·20 cubic inches.

The quantity of carbonic acid gas produced was 18·20 cubic inches.

18·2 cub. inch. carbonic acid produced,

100 : 47·26 : : 18·20 : : 8·60 grains.

8·60 : 2·49 : : 100 : 28·95.

Then 100 grains of carbonic acid gas contain 28·95 of diamond.

containing ·2895 by wt. of diamond.

Q 2 Calculation

100 : 33·82 : : 18·20 : 6·15 grains of oxigen consumed
2·49 grains of diamond.

8·64

Calculation by carbonic acid 8·60

·04 difference.

8·64 : 2·49 : : 100 : 28·81.

or from the oxigen consumed, ·2881.

Thus, if we calculate upon the oxigen consumed, 100 grains of carbonic acid gas contain 28·81 of diamond.

Second Experiment on Diamond.

Exp. 2.

Thermometer 48° Fahrenheit, barometer 30·08. Oxigen gas, made as usual, left a residuum of 3 parts in 100.

4·01 grs. of diamonds

Eleven small diamonds, weighing 4·01 grains, were put into the tray. We began with 49·84 cubic inches of oxigen; and every thing being properly adjusted, kept the platina tube red-hot for a quarter of an hour, and during this time the gas was passed from one gasometer to the other, as in the former experiments. When the tubes, &c. were cooled down to the temperature of the room, all the gas was transferred to gasometer No. 1, and the volume was exactly the same as before the experiment. On examining the tray, all the diamonds were entirely confumed and not a vestige left.

entirely consumed.

Lime water absorbed 57·5 parts from 100
The test for oxigen 39·5
Residuum ········ 3

100

Correction for temperature.

60°	0·103	49·84
48	12	1·23
12 diff.	1·236 add for temp.	51·07

Correction for pressure.

30 : 30·08 : : 51·07 : 51·20.

The

The volume of gas at the mean was therefore 51·20 cubic inches.

100 : 57·50 : : 51·20 : 29·44.

Then 29·44 cubic inches of carbonic acid gas were produced. Produced 29·44 cub. in. of carbonic acid gas

100 : 47·26 : : 29·44 : 13·91.
13·91 : 4·01 : : 100 : 28·82.

Then, according to this experiment, 100 grains of carbonic acid contain 28·82 diamond. containing ·2882 of diamond,

Calculation by Oxigen.

100 : 33·82 : : 29·44 : 9·95 grains of oxigen consumed
4·01 of diamond

13·96
Calculation by carbonic acid 13·91

·05 diff.

13·96 : 4·01 : : 100 : 28·72.

Then, calculating by the weight of oxigen employed, 100 grains of carbonic acid contain 28·72 diamond. or, from oxigen consumed ·2872.

The precipitate in lime water from the gas produced in the combustion of diamond appeared to us denser than that from the combustion of charcoal. Appeared to occasion a denser precipitate in limewater, than that from charcoal.

In order to see how far the weight of the precipitate of carbonate of lime would agree with the results of the foregoing experiments, we drew off 20·5 cubic inches of the gas, which had been thus altered by the combustion of diamond in the last experiment, by the register H, and received it in bottles over mercury; then admitting lime water, we obtained a copious precipitate of carbonate of lime, which, being dried at the temperature of 212° Fahrenheit, weighed 12 grains.

But as the 20·5 cubic inches require the same corrections to bring them to the mean temperature and pressure; we say, as the actual volume of all the gas is to its correction, so is the quantity drawn off to that which it would have been at the mean:

49·84 : 51·20 : : 20·50 : 21·06, the volume after the corrections were made.

Then,

Then, to find how much carbonic acid was contained in these 21·06 cubic inches, we state it thus: As the total quantity of gas after the experiment is to the total weight of carbonic acid gas found by calculation, so is the quantity of gas experimented upon to the weight of carbonic acid gas which it ought to have contained,

51·20 : 13·91 : : 21·06 : 5·72 grains.

Every 100 grains of precipitated carbonate of lime contain 44 grains of carbonic acid; 12 grains were procured in our experiment. 100 : 44 : : 12 : 5·28

The weight of the precipitate agreed nearly with the foregoing results.

Therefore the carbonic acid contained in our precipitate of 12 grains weighed 5·28; by calculation it should have weighed 5·72; this is as near as we had a right to expect from the difficulty of collecting the precipitate.

Stone Coal.

Experiment with Welch stone coal.

Upon the suggestion of our mutual friend Professor Davy, we next examined the results of the combustion of stone coal and plumbago; thermometer 57° Fahrenheit, barometer 29·65.

The stone coal from Wales, employed by maltsters, is well known to contain little or no maltha, or mineral pitch, and to burn without flame.

4 grs. charred and then burned.

A portion of this coal was placed under sand in a crucible, and exposed to a strong heat for one hour; 4 grains of it thus prepared were put into the tray: our oxigen left a residuum of 5 parts in 100, and we began with 49·84 cubic inches as usual. The tray being placed in the platina tube was heated to redness for about 10 minutes. When the gas was first passed. we thought we saw a flash in the glass tubes. On suffering the whole to cool, the quantity of gas still remained the same, and the tray being drawn out contained only ·5 of a grain unconsumed. From the gas thus charged with 3·5 grains of coal,

½ gr. residuum.

Residual gas increased ·03

Lime water absorbed	53	parts from 100.
The tests for oxigen	39	
Residuum	8	or an increase of 3.
	100	

Correction

Correction for temperature.

60°	0·103	49.84
57	3	·30
3 diff.	0·309 add for temp.	50·14

Correction for pressure,

30 : 29·65 : : 50·14 : 49·55.

The quantity of oxigen at the mean was therefore 49·55 cubic inches.

100 : 53 : : 49·55 : 26·26.

Consequently 26·26 cubic inches of carbonic acid gas were produced. 26·26 cubic inch carbonic acid gas produced,

100 : 47·26 : : 26·26 : 12·41 grains.

12·41 : 3·50 : : 100 : 28·20.

Then, according to this experiment, 100 grains of carbonic acid gas contain 28·20 of coal. containing ·2820 of coal,

Calculation by oxigen.

100 : 33·82 : : 26·26 : 8·88 grains of oxigen consumed.

3·50 coal

12·38

Calculation by carbonic acid	12·41
by oxigen	12·38
difference	·03

Here, contrary to what happened in other experiments, the calculation by carbonic acid rather exceeds that by oxigen: or, from oxigen consumed; ·2827.

12·38 : 3·50 : : 100 : 28·27.

Calculating therefore by oxigen, 100 grains of carbonic acid contain 28·27 of coal.

Experiment with Plumbago.

Thermometer 44° Fahrenheit, barometer 29·94. Exp. with plumbago. 4 grs.

Four grains of plumbago, from a very fine specimen be-

longing

longing to Dr. Babingtou, were put into the tray. Our oxigen left a residuum of 2 parts in 100, and we began with 49·84 cubic inches. The tray, with its contents, being placed in the platina tube, was heated to redness for a quarter of an hour, and the gas made to pass over it several times. When all was cool, the original quantity was neither increased nor diminished, and on withdrawing the tray we found only ·2 of a grain of oxide of iron; so that this specimen of plumbago contains only 5 per cent oxide of iron.

left ·2 gr. of oxide of iron.

The gas being now examined,

Lime water absorbed	55 parts from 100	
The tests for oxigen	42	
Residuum	3	or an increase of 1 per cent.
	100	

Residual gas increased ·01.

Correction for temperature.

60°	0·103	49·84
44°	16	1·64
16 diff.	1·648 add for temp.	51·48

Correction for pressure.

30 : 29·94 : : 51·48 : 51·37.

The quantity of oxigen at the mean would be 51·37 cubic inches.

100 : 55 : : 51·37 : 28·25.

Therefore 28·25 cubic inches of carbonic acid gas were produced.

100 : 47·26, : : 28·25 : 13·35 grains.

13·35 : 3·8 : : 100 : 28·46.

28 25 cubic inches of carbonic acid gas produced, containing ·2846 of carbon.

Then, according to this experiment, 100 grains of carbonic acid contain 28·46 of the carbonaceous part of the plumbago.

Calculation by oxigen.

100 : 33·82 : : 28·25 : 9·55 grains of oxigen consumed 3·80 plumbago.

13·35

Calculation by carbonic acid 13.35

First

First Experiment on animal Charcoal.

Thermometer 60° Fahrenheit, barometer 30·23. Exp. 1. On animal charcoal,

Muscular fibre distilled in a coated glass retort left a black shining coal, 4 grains of which were put into the tray. Our oxigen left a residuum of 2 parts in 100. The tray and its contents being placed in the platina tube, was heated to redness for 8 minutes. The first time the gas was passed, a lambent flame filled the whole length of the glass tube, and the gas became turbid or milky. It was passed frequently through the heated tube, but we observed no repetition of the flashes. Hence we conjecture that if the diamond had contained hidrogen, we should probably have had a similar appearance. After the experiment all the apparatus was, as usual, perfectly tight, and the volume of gas unaltered. On examining the platina tray a minute portion of charcoal remained, and a quantity of saline matter adhered to it so firmly, that it became difficult to ascertain the quantity of carbon consumed, and we forebore to make the calculation; we however examined the gas.

4 grains from muscular fibre.

Lambent flame.

Gas rendered turbid.

Saline matter left.

Lime water absorbed	40 parts from 100	
The tests for oxigen	54	
Residuum	6 or an increase of 4 per ct.	Residual gas increased ·04.
	100	

Second Experiment on animal Charcoal.

Thermometer 59° Fahrenheit, barometer 29·45. Exp. 2 On animal charcoal.

Some of the animal charcoal of the last experiment was heated to redness under sand for one hour. Four grains were placed in the platina tray; and as we were so much embarrassed in the last experiment with the saline matter which adhered to the tray, we exactly balanced it with its contents. Our oxigen, made as usual, left a residuum of 2 parts in 100, and we began with 49·84 cubic inches. When every thing was adjusted, and the platina tube red hot, on passing the oxigen, flashes resembling lightning ran along the glass tube; and this was repeated 5 or 6 times. The whole of the gas became very cloudy, exhibiting a turbid milky

4 grs.

Flashes of light.

Gas turbid.

milky appearance. The tube was rendered white hot by the combustion of the carbonaceous matter in oxigen. The fire was kept up about 8 minutes, and the gas passed several times. When all was cool, we could observe no alteration in the volume of gas by the register. The tray contained a mixture of salts; and being weighed, was lighter by 3·2 grains. This loss was not wholly carbon, for it is well known that animal substance contains a variety of salts, as phosphates, muriates, &c. some of which, though not volatile in a low red heat, might be decomposed and dissipated in the intense white heat produced by the combustion of the carbonaceous matter in oxigen; and we accordingly found the internal parts of the gasometers and tubes very slightly covered with a sort of efflorescence. On examining the gas after the experiment,

Residuum ·8 grs.

Slight efflorescence on the interior of the apparatus.

Lime water absorbed	41	parts from 100
The tests for oxigen	55	
Residuum	4	or an increase of 2.
	100	

Residual gas increased ·02.

Correction for temperature.

60°	49·84	
59	·10	add for temp.
1 diff, or 0·103	49·94	

Correction for pressure.

30 : 29·45 : : 49·94 : 49·02.

The quantity of oxigen at the mean would therefore be 49·02 cubic inches.

100 : 41 : : 49·02 : 20·09

The carbonic acid gas produced was therefore 20·09 cubic inches.

Carbonic acid gas produced 20 09 cub. in.

100 : 47·26 : : 20·09 : 9·49

and this carbonic acid weighed 9·49 grains.

Now the coal in the tray had lost 3·2 grains; but as the whole of this was not carbon, but part of it volatile saline matter,

matter, &c. we shall endeavour to estimate the carbon by the experiment on plumbago. When 13·35 grains of carbonic acid contained 3·80 grains of carbon,

$$13{\cdot}35 : 3{\cdot}80 :: 9{\cdot}49 : 2{\cdot}70.$$

The quantity of carbonic acid produced in this experiment, therefore, contained 2·70 grains of carbon. Containing 2·7 grs. of carbon.

Loss	3·20
Carbon	2·70
Leaves	·50 for volatile saline matter, &c.

So that, this being granted, the present experiment agrees with the foregoing. Matter volatilized ·5 grs.

In two of our first experiments with box-wood charcoal, the calculations gave us in one case 29·75 parts of carbon in 100 of carbonic acid, and the other 30·68; but we were not then fully aware of the absorption of water by charcoal, which rendered the quantity of real carbon employed less than indicated by the weight. Also in another experiment, in which 4 grains of diamond were consumed, the calculation gave us 29·96 per cent of diamond in carbonic acid; but apprehending, that a slight degree of inaccuracy had crept into this experiment, we have not detailed it with the rest; but we have thought it right to give a simple statement of matters of fact; in no one instance have we endeavoured to strain or accommodate these to suit any particular theory, being fully aware, that every experiment, carefully made and faithfully recorded, will remain an immutable truth to the end of time, while hypotheses are constantly varying, and even the most beautiful theories are liable to change. In some experiments quantity of carbon apparently greater.

The experiments above related give us the following results. Table of results.

	By carbonic acid.	By oxigen.
Box-wood charcoal	28·92	28·77
1st expt. diamond	28·95	28·81
2d expt. diamond	28·82	28·72
Stone coal	28·20	28·27
Plumbago	28·46	28·46
	5)143·35	5)143·03
mean	28·67	28·60

Hence,

100 grs. carbonic acid contain 28·6 of carbon.

Hence we conclude, that 100 grains of carbonic acid contain 28·60 of carbon, which does not greatly differ from the results of the experiments of Smithson Tennant, Esq. on the nature of diamond. See Phil. Trans. 1797.

Mr. Tennant's experiments

This gentleman made his experiment in the following manner. A quarter of an ounce of nitrate of potash was rendered somewhat alkaline by exposure to heat, in order that it might more readily absorb carbonic acid; it was then put into a gold tube with $2\frac{1}{2}$ grains of diamond, and being subjected to heat, the diamond was converted into carbonic acid, by uniting with the oxigen contained in the nitric acid. The carbonic acid thus produced combined with the potash, and on pouring a solution of muriate of lime into a solution of this salt, he obtained a precipitate of carbonate of lime, this, being decomposed by muriatic acid, gave as much carbonic acid gas as occupied the space of 10·1 ounces of water. The thermometer was at 55° Fahrenheit, the barometer 29·80. In a second experiment he procured a larger quantity, or equal to 10·3 ounces of water.

gave 27 or 27·8.

If we therefore consider an ounce of water as consisting of 480 grains, and a cubic inch of water equal to 253 grains, and then make the proper corrections for temperature and pressure, one of his experiments will give about 27 per cent, the other about 27·80 for the carbon in carbonic acid, which is somewhat less than our estimate; but the difference may easily be accounted for, from the different methods employed.

Guyton's experiments not to be depended on.

The experiments of Guyton, as detailed in the *Annales de Chimie*, vol. XXXI, page 76, are liable to very strong objections; but at the same time the candid manner, in which he has related every circumstance, merits considerable praise. It is impossible, however, not to observe, that the quantity of gas before and after the experiment could not, from the construction of his apparatus, be very rigorously ascertained. We object also to nitrous gas as a test for oxigen; and as it is acknowledged, that the wooden support included in the oxigen gas took fire, the product of carbonic acid must have been influenced by it; so that, if no chance of errour had existed in estimating the carbonic acid gas from the residuum after barytic water had absorbed a part, still the result would not have been satisfactory.

The

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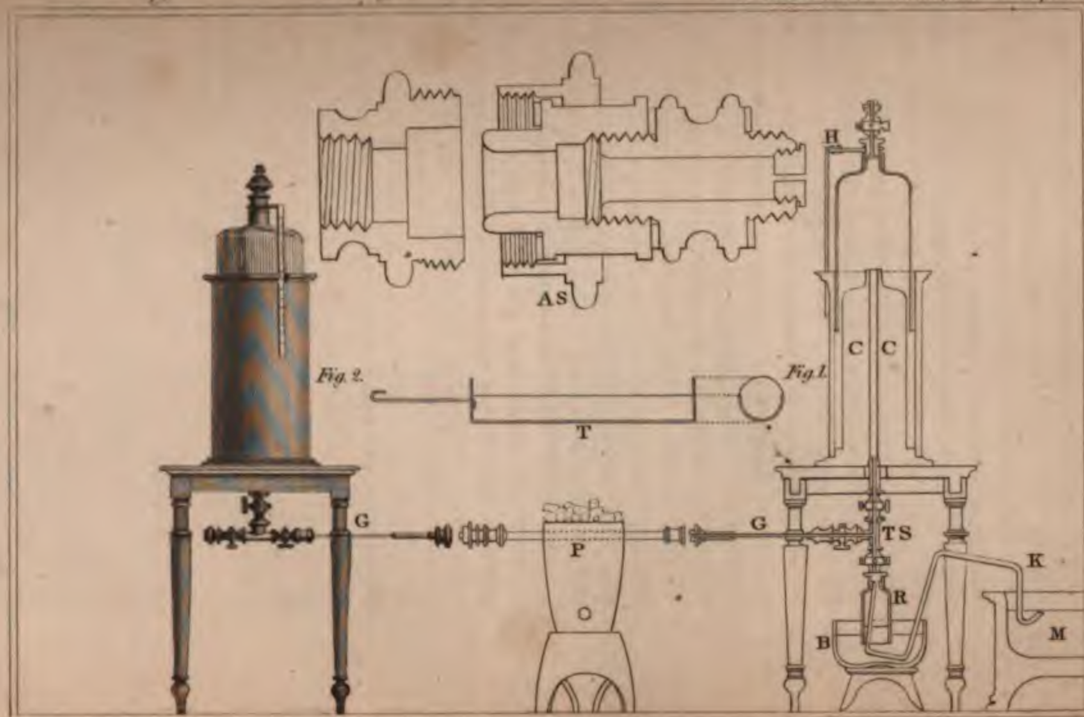
Messrs. Allen & Pepys on Carbonic Acid & the Diamond.

Nicholson's Philos. Journal, Vol. XIX, Pl. VI, p. 240.



The experiments which we have had the honour of laying before this Society prove several important points: General conclusions.

1st. That the estimate given by Lavoisier, of 28 parts of carbon in every 100 parts of carbonic acid, is very nearly correct; the mean of our experiments makes it 28·60. Lavoisier's estimate nearly correct.

2dly. That the diamond is pure carbon; for had it contained any notable proportion of hidrogen, it must have been discovered, either by detonating with the oxigen, as in the case of animal charcoal, or by diminishing the quantity of oxigen gas. Diamond pure carbon.

3dly. That well burnt charcoal contains no sensible quantity of hidrogen; but if exposed to the air for a few hours it absorbs moisture, which renders the results uncertain. Fresh charcoal contains no hidrogen.

4thly. That charcoal can no longer be considered as an oxide of carbon, because, *when properly prepared*, it requires quite as much oxigen for its combustion as the diamond. This is also the case with stone coal and plumbago. Charcoal not oxide.

5thly. It appears that diamond and all carbonaceous substances (as far as our present methods of analysis are capable of demonstrating their nature) differ principally from each other in the state of aggregation of their particles. Berthollet has well remarked, that in proportion as this is stronger, decomposition is more difficult: and hence the variety of temperatures required for the combustion of different inflammable substances. Carbonaceous substances differ only in their aggregation.

IX.

Account of an extinct Volcano in Britain. Communicated by Mr. Donovan.

MR. Donovan announces some particulars of an extraordinary nature to the scientific world respecting one of the Cambrian mountains; which, from the result of attentive observation, and indubitable evidence, he endeavours to demonstrate must have been at some remote period a volcano of Cader Idris formerly a volcano.

of immense magnitude. The mountain alluded to is Cader Idris, situate in the county of Merioneth, which in point of size is esteemed the most considerable in the principality of Wales, Snowdon alone excepted.

First noticed by Mr. Donovan seven years ago.

The remarkable appearance of this stupendous mountain attracted the attention of Mr. Donovan about seven years ago. He was then led to consider from a variety of circumstances, that the original form of the mountain must have undergone very material alteration, occasioned as he conceived by the powerful effects of the volcanic explosion; but his remarks were not sufficiently precise to authorise the assertion. Since that period he has examined the mountain in a less cursory manner, more especially in the summer of 1807, when he was at full leisure to devote some time to this interesting subject of inquiry, and his observations in the latter instance tend entirely to confirm the idea first suggested. In support of this opinion Mr. Donovan has now added to his museum abundant examples of different kinds of lava, pumice, and other volcanic matters of the most unequivocal character, collected by himself from the sides and base of the mountain; and also a suite of the remarkable and singularly formed columnar crystals of basalt, that are scattered in profusion about the loftiest summit, and cliffs surrounding the crater.

Proofs in volcanic productions collected there in 1807.

Appearance of the crater.

The general aspect of this crater is exactly that of mount Vesuvius, except that one of its sides is broken down, by which means the abyss of this funnel-shaped excavation is more completely disclosed than in the Vesuvian mountain; and it this side of Cader Idris which affords the most illustrative examples of porous stones, these forming immense beds on the declivities a few inches only in many instances below the surface of the earth. A number of these porous stones lately found in this spot by Mr. Donovan exhibit evident marks of strong ignition and vitrification, some are reduced to the state of slags, while others have all the cellular appearance and lightness of pumice.

The crater formed by an explosive power.

Without entering upon any discussion as to the relative merits of the neptunian and vulcanian theories, it must be admitted, that the agency of water might have contributed materially to affect those changes in the primitive form of the Cader

Cader Idris mountain, which have evidently taken place. But with respect to the crater itself, this appears very clearly to have derived its origin from the violence of an explosion upwards, in which a very considerable portion of the highest eminence was torn from its native bed of rocks, and thrown to a considerable height over the other parts of the mountain. In confirmation of this suggestion it should be mentioned, that the summit of the mountain is covered with an immense wreck of the stones, ejected as it is presumed from the crater at the time of this explosion; it would be difficult otherwise to account for the vast profusion of those stones scattered in all directions about the loftiest elevations, and which, from the confused manner in which they are dispersed, must have been thrown into their present situation by no small violence. Myriads of these stones have borne a regular crystallized form, though from their great bulk and weight they have for the most part suffered material injury in the general convulsion. The usual length of these crystals is from three to six or ten feet in length: some measure even fifteen or twenty, and one in particular, which Mr. Donovan has seen, was twenty-two feet three inches long. They are however slender in proportion to the length.

Proofs of this.

Columnar stones from 3 to 22 feet long.

The substance of these crystals is of the basalt kind, and corresponds very nearly with some varieties of the "*lave porphyre*" of Etna described by Dolomieu, and Faujas de St. Fond; and in the form of its crystals agrees with others of the *basaltes prismatique* of the last author. In the neptunian theory it is not indeed admitted as a basalt, but as a porphyry argil. It is the *porphir-schiefer* of Werner, and porphyry slate, or clinkstone porphyry of Jamieson.

Basaltes.

The suite of these stupendous crystals, which Mr. Donovan collected from the summit of Cader Idris last summer, and has lately added to his museum, consists of a small trihedral column about eighteen inches in length; a tetrahedral column of much superior size; an interesting portion of a pentagonal column, and another of the same figure about four feet in length, and having the termination of the crystal complete. The latter is estimated at about five hundred weight, but this is still exceeded by another of a somewhat compressed hexagonal figure with an oblique termination.

Specimens of it in Mr Donovan's museum.

The

The whole of these are very perfect, and extremely well defined.

Lambeth, Feb. 22*d.* 1808.

SCIENTIFIC NEWS.

New mode of preparing Calomel.

THE object of this process, invented by Mr. Joseph Jewel, is to produce a calomel, that shall always be in the state of an impalpable powder. In the common mode this is effected by grinding, or trituration, which is liable to be negligently performed: and Mr. Jewel, to prevent all danger of this, endeavours to obtain it in a powder uniformly fine, by a particular manipulation in the last sublimation of the calomel, which he describes as follows.

I take calomel or mercurius dulcis, broken into small pieces, and put it into an earthen crucible of the form of a long bowl, so as to fill about one half thereof. I place the crucible on its side in a furnace provided with an opening, through which the mouth of the crucible projects about an inch. I then join to the mouth of the crucible an earthen ware receiver, having an opening at its side, to receive the open end of the crucible. This receiver is about half filled with water. I lute the joint with a mixture of sand and pipe clay. The receiver has a cover, which cover has a side continued upwards for containing water, with a chimney or tube in it, to allow the escape of steam from the water below. I then apply a fire around the crucible, sufficient to raise the calomel in vapours, and forced it through the mouth of the crucible into the receiver; where, by the water while cold, or assisted by the steam when it becomes hot, it is instantly condensed into an impalpable powder, possessing all the qualities of calomel in its most perfect state. The calomel, when thus prepared, is purer, whiter, and more attenuated, than that obtained by grinding. It is proper to wash the product over with water, before it is dried, to rid it of any coarser particles, which may form about the mouth of the crucible.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

APRIL, 1808.

ARTICLE I.

On the formation of the Bark of Trees. In a Letter from T. A. KNIGHT, *Esq. F. R. S. to the Right Honourable Sir* JOSEPH BANKS, K. B. P. R. S. *&c**.

MY DEAR SIR,

AN extraordinary diversity of opinion appears to have prevailed among naturalists, respecting the production and subsequent state of the bark of trees.

Various opinions respecting the production of bark.

According to the theory of Malpighi, the cortical substance, which is annually generated, derives its origin from the older bark; and the interior part of this new substance is annually transmuted into alburnum or sap wood; whilst the exterior part, becoming dry and lifeless, forms the exterior covering or cortex.

The opinions of Grew do not appear to differ much from those of Malpighi; but he conceives the interior bark to consist of two distinct substances, one of which becomes albur-

* Philos. Tras. for 1806, Part 1, p. 103. Sir Godfrey Copley's gold medal for 1806 was adjudged to Mr. Knight for his various papers on vegetation printed in the Phil. Trans.

num, whilst the other remains in the state of bark: he, however, supposes the insertments in the wood, the "utriculi" of Malpighi, and the "tissu cellulaire" of du Hamel, to have originally existed in the bark.

Hales on the contrary contends, that the bark derives its existence from the alburnum, and that it does not undergo any subsequent transformation.

The discoveries of du Hamel have thrown much light on the subject; but his experiments do not afford any conclusive result, and some of them may be adduced in support of either of the preceding hypotheses: and a modern writer (Mirbel*) has endeavoured to combine and reconcile, in some degree, the apparently discordant theories of Malpighi and Hales. He contends with Hales, that the alburnum gives existence to the new layer of bark; but that this bark subsequently changes into alburnum, though not precisely in the manner described by Malpighi.

So much difference of opinion, amongst men so capable of observing, sufficiently evinces the difficulty of the subject they endeavoured to investigate: and in a course of experiments, which has occupied more than twenty years, I have scarcely felt myself prepared, till the present time, even to give an opinion respecting the manner in which the cortical substance is generated in the ordinary course of its growth; or reproduced, when that, which previously existed, has been taken off.

Bark of some trees reproduced,

Du Hamel has shown, that the bark of some species of trees is readily reproduced, when the decorticated surface of the alburnum is secluded from the air; and I have repeated similar experiments on the apple, the sycamore, and other trees, with the same result; I have also often observed a similar reproduction of bark on the surface of the alburnum of the *wych elm* (ulmus montana) *in shady situations*, when no covering whatever was applied. A glareous fluid, as du Hamel has stated, exudes from the surface of the alburnum:

apparently from the alburnum.

this fluid appears to change into a pulpous unorganized mass, which subsequently becomes organized and cellular; and the

* Traité d'Anatomie et de Physiologie végétale.

matter,

matter, which enters into the composition of this cellular substance, is evidently derived from the alburnum.

These facts are therefore extremely favourable to the theory of Hales; but other facts may be adduced, which are scarcely consistent with that theory.

Internal surface of the bark itself generates it more readily.

The internal surface of pieces of bark, when detached from contact with the alburnum, provided they remain united to the tree at their upper ends, much more readily generate a new bark, than the alburnum does under similar circumstances: a similar fluid exudes from the surfaces of both, and the same phenomena are observable in both cases. The cellular substance, however, which is thus generated, though it presents every external appearance of a perfect bark, is internally very imperfectly organized; and the vessels which contain the true sap in the bark are still wanting; and I have found, that these may be made, by appropriate management, to traverse the new cellular substance in almost any direction. When I cut off all communication above, and on one side, between the old bark and that substance, I observed, that the vessels proceeded across it, from the old bark on the other side, taking always in a greater or less degree an inclination downwards; and when the cellular substance remained united to the bark at its upper end only, the vessels descended nearly perpendicularly down it; but they did not readily *ascend* into it, *when it was connected with the bark at its lower extremity only;* the result of similar experiments, when made on different species of trees, was, however, subject to some variations.

Course of the new vessels variable.

Experiment on the walnut.

Pieces of bark of the walnut-tree, which were two inches broad, and four long, having been detached from contact with the alburnum, except at their upper ends, and covered with a plaster composed of bees-wax and turpentine, in some instances, and with clay only in others, readily generated the cellular substance of a new bark; and between that and the old detached bark, very nearly as much alburnum was deposited as in other parts of the tree, where the bark retained its natural position; which, I think, affords very decisive evidence of the descent of the sap through the bark. Similar pieces of bark, under the same mode of treatment, but

The sap descends through the bark.

but united to the tree at their lower ends only, did not long remain alive, except at their lower extremities; and there a very little alburnum only was generated. Other pieces of bark of the same dimensions, which were laterally united to the tree, continued alive almost to their extremities: and a considerable portion of alburnum was generated, particularly near their lower edges; the sap appearing in its passage across the bark to have been given a considerable inclination downwards: probably owing to an arrangement in the organization of the bark, that I have noticed in a former memoir*, which renders it better calculated to transmit the sap towards the roots than in any other direction.

Bark reproduced from the alburnum of the sycamore and apple.

I have in very few instances been able to make the walnut-tree reproduce its bark from the alburnum, though under the same management I rarely failed to succeed with the sycamore and apple tree. Pieces of the bark of the apple-tree will also live, and generate a small portion of alburnum, though only attached to the tree at their lower extremities; probably owing to a small part of the true sap being carried upwards by capillary attraction, when the proper action of the cortical vessels is necessarily suspended.

The preceding experiments, and the authority of du Hamel, having perfectly satisfied me, that both the alburnum and the bark of trees are capable of generating a new bark, or at least of transmitting a fluid capable of generating a cellular substance, to which the bark in its more perfectly organized state owes its existence, my attention was directed to discover the sources from which this fluid is derived.

Both bark and alburnum consist of tubes and cellular substance.

Both the bark and the alburnum of trees are composed principally of two substances; one of which consists of long tubes, and the other is cellular; and the cellular substance of the bark is in contact with the similar substance in the alburnum, and through these I have long suspected the true sap to pass from the vessels of the bark to those of the alburnum†. The intricate mixture of the cellular and vascular substances long baffled my endeavours to discover from which

* Philosophical Transactions of 1804, or Philosophical Journal, vol. X, p. 289.

† Phil. Trans. 1805, p. 14, or Philos. Journal, vol. XIII, p. 352.

of

of them, in the preceding cases, the sap, and consequently the new bark, proceeded; but I was ultimately successful.

The cellular substance, both in the alburnum and bark of old pollard oaks, often exists in masses of near a line in width, and this organization was peculiarly favourable to my purpose. I therefore repeated on the trunks of trees of this kind experiments similar to those above mentioned, which were made on the walnut-tree.

Experiments on pollard oaks.

Apparently owing to the small quantity of sap, which the old pollard trees contained, their bark was very imperfectly reproduced; but I observed a fluid to ouze from the cellular substance, both of the bark and alburnum; and on the surface of these substances alone, in many instances, the new bark was reproduced in small detached pieces.

I have endeavoured to prove in former communications*, that the true sap of trees acquires those properties which distinguish it from the fluid recently absorbed, by circulating through the leaf; and that it descends down the bark, where part of it is employed in generating the new substances annually added to the tree; and that the remainder, not thus expended, passes into the alburnum, and there joins the ascending current of sap. The cellular substance, both of the bark and alburnum, has been proved, in the preceding experiments, to be capable of affording the sap a passage through it; and therefore it appears not very improbable, that it executes an office similar to that of the anastomosing vessels of the animal economy, when the cellular surfaces of the bark and alburnum are in contact with each other; and, when detached, it may be inferred, that the passing fluid will exude from both surfaces: because almost all the vessels of trees appear to be capable of an inverted action in giving motion to the fluids which they carry.

Absorbed fluid converted into sap by circulation in the leaf, and then descends down the bark, to form new substance.

As the power of generating a new bark appeared in the preceding cases to exist alike in the sap of the bark and of the alburnum, I was anxious to discover how far the fluid, which ascends through the central vessels of the succulent annual shoot, is endued with similar powers. Having there-

Sap ascending in annual shoots can generate new bark.

* Phil. Trans 1801, 1805, and 1806.

fore

fore made two circular incisions through the bark, round the stems of several annual shoots of the vine, as early in the summer as the alburnum within them had acquired sufficient maturity to perform its office of carrying up the sap, I took off the bark between these incisions; and I abraded the surface of the alburnum to prevent a reproduction of it. The alburnum in the decorticated spaces soon became externally dry and lifeless; and several incisions were then made longitudinally through it. The incisions commenced a little above, and extended below the decorticated spaces, so that, if the sap of the central vessels generated a cellular substance (as I concluded it would), that substance might come into contact and form a union with the substance of the same kind emitted by the bark above and below.

The experiment succeeded perfectly, and the cellular substances generated by the central vessels, and the bark, soon united, and a perfect vascular bark was subsequently formed beneath the alburnum, and appeared perfectly to execute the office of that which had been taken off; the medulla appeared to be wholly inactive.

Cortical vessels from regenerated buds spread in various directions.

I have already observed, that the vessels, which were generated in the cellular substance on the surface of the alburnum of the sycamore and the apple-tree, traversed that substance in almost every direction; and the same thing appears to occur beneath the old bark, when united to the alburnum. For having attentively examined, through every part of the spring and summer, the formation of the internal bark, and alburnous layer beneath it, round the bases of regenerated buds, which I had made to spring from smooth spaces on the roots and stems of trees, I found every appearance perfectly consistent with the preceding observations. A single shoot only was suffered to spring from each root and stem, and from the base of this, in every instance, the cortical vessels dispersed themselves in different directions. Some descended perpendicularly downwards, whilst others diverged on each side, round the alburnum, with more or less inclination downwards, and met on the opposite side of it. The same pulpous and cellular substance appeared to cover the surfaces of the bark and alburnum, when in contact with each

each other, as when detached; and through this substance the ramifications of the vessels of the new bark extended themselves, appearing to receive their direction from the fluid sap, which descended from the bark of the young shoots, and not to be, in any degree, influenced in their course by the direction taken by the cortical and alburnous vessels of the preceding year.

Cortical vessels meeting form an interwoven texture.

Whenever the vessels of the bark, which proceeded from different points, met each other, an interwoven texture was produced, and the alburnum beneath acquired a similar organization; and the same thing occurs, and is productive of very important effects, in the ordinary course of the growth of trees. The bark of the principal stem, and of every lateral branch, contains very numerous vessels, which are charged with the descending true sap; and at the juncture of the lateral branch with the stem, these vessels meet each other. A kind of pedestal of alburnum, the texture of which is much interwoven, is in consequence formed round the base of the lateral branch; which thus becomes firmly united to the tree. This pedestal, though apparently a part of the branch, derives a large portion of the matter, annually added to it, from the cortical vessels of the principal stem; and thence, in the event of the death of the lateral branch, it always continues to live. But it not unfrequently happens, that a lateral branch forms a very acute angle with the principal stem, and, in this case, the bark between them becomes compressed and inactive; no pedestal is in consequence formed, and the attachment of such a branch to the stem becomes extremely feeble and insecure*. Instead of

Junction of lateral branches.

Weak when forming a very acute angle.

Advantages of properly pruning and training timber trees.

* The advantages, which may be obtained by pruning timber trees judiciously, appear to be very little known. I have endeavoured to ascertain the practicability of giving to trees such forms as will render their timber more advantageously convertible to naval or other purposes. The success of the experiments, on small trees, has been complete, and the results perfectly consistent, in every case, with the theory I have endeavoured to support in former memoirs; and I am confident, that by appropriate management, the trunks and branches of growing trees may be moulded into the various forms best adapted to the use of the ship-builder; and that the growth of the trees may at the same time be rendered considerably more rapid, without any expense or temporary loss to the proprietor.

the reproduced buds of the preceding experiment, buds were inserted in the foregoing summer, or attached by grafting in the spring; and, when these succeeded, though they were in many instances taken from trees of different species, and even of different genera, no sensible difference existed in the vessels, which appeared to diverge into the bark of the stock, from these buds and from those reproduced in the preceding experiments.

Theory.

It appears, therefore, probable, that a pulpous organizable mass first derives its matter either from the bark, or the alburnum; and that this matter subsequently forms the new layer of bark; for, if the vessels had proceeded, as radicles*, from the inserted buds or grafts, such vessels would have been in some degree different from the natural vessels of the bark of the stocks; and it does not appear probable, even without referring to the preceding facts, that vessels should be extended, in a few days, by parts successively added to their extremities, from the leaves to the extremities of the roots; which are, in many instances, more than two hundred feet distant from each other. I am, therefore, inclined to believe, that, as the preceding facts seem to indicate, the matter, which composes the new bark, acquires an organization calculated to transmit the true sap towards the roots, as that fluid progressively descends from the leaves in the spring; but whether the matter, which enters into the composition of the new bark, be derived from the bark or the alburnum, in the ordinary course of the growth of the tree, it will be extremely difficult to ascertain.

Bark sometimes exists previous to the alburnum.

It is, however, no difficult task to prove, that the bark does not, in all cases, spring from the alburnum; for many cases may be adduced, in which it is always generated previously to the existence of the alburnum beneath it; but none, I believe in which the external surface of the alburnum exists previously to the bark in contact with it, except when the cortical substance has been taken off, as in the preceding experiments. In the radicle of germinating seeds, the cortical vessels elongate, and new portions of bark are succes-

* Darwin's Phytologia.

sively

sively added to their points, many days before any alburnous substance is generated in them; and in the succulent annual shoot the formation of the bark long precedes that of the alburnum. In the radicle the sap appears also evidently to descend* through the cortical vessels†, and in the succulent annual shoot it as evidently passes up through the central vessels‡, which surround the medulla. In both cases a cellular substance, similar to that which was generated in the preceding experiments, is first formed, and this cellular substance in the same manner subsequently becomes vascular; whence it appears, that the true sap, or blood of the plant, produces similar effects, and passes through similar stages of organization, when it flows from different sources, and that the power of generating a new bark, properly speaking, belongs neither to the bark nor alburnum, but to a fluid, which pervades alike the vessels of both.

Bark not transmuted into alburnum.

I shall, therefore, not attempt to decide on the merits of the theory of Malpighi, or of Hales, respecting the reproduction of the interior bark; but I cannot by any means admit the hypothesis of Malpighi and other naturalists, relative to the trasmutation of bark into alburnum; and I propose to state my reasons for rejecting that hypothesis, in the next communication I have the honour to address to you.

I am, my dear Sir,

Your most obliged obedient Servant,

Elton, Dec. 18, 1806. T. A. KNIGHT.

* Phil. Trans. 1805 and 1806, or Philos. Journal, vol. XIII and XVI.

† I wish it to be understood, that I exclude in these remarks, and in those contained in my former Memoirs, all trees of the palm kind, with the organization of which I am almost wholly unacquainted.

‡ Phil. Trans. 1805. Mirbel has called the tubes, which I call the central vessels, the "tissu tubulaire" of the medulla.

II.

II.

On the Economy of Bees. In a Letter from THOMAS ANDREW KNIGHT, *Esq. F.R.S.* to *the Right Honourable Sir* JOSEPH BANKS, *Bart. K.B. P.R.S.**

MY DEAR SIR,

IN the prosecution of those experiments on trees, accounts of which you have so often done me the honour to present to the Royal Society, my residence has necessarily been almost wholly confined to the same spot; and I have thence been induced to pay considerable attention to the economy of bees, amongst other objects; and as some interesting circumstances in the habit of these singular insects appear to have come under my observation, and to have escaped the notice of former writers, I take the liberty to communicate my observations to you.

Friendly intercourse takes place between bees of different swarms.

It is, I believe, generally supposed, that each hive, or swarm, of these insects remains at all times wholly unconnected with other colonies in the vicinity; and that the bee never distinguishes a stranger from an enemy. The circumstances which I shall proceed to state will, however, tend to prove, that these opinions are not well founded, and that a friendly intercourse not unfrequently takes place between different colonies, and is productive of very important consequences in their political economy.

Evening visits between two hives.

Passing through one of my orchards rather late in the evening in the month of August, in the year 1801, I observed, that several bees passed me in a direct line from the hives in my own garden to those in the garden of a cottager, which was about a hundred yards distant from it. As it was considerably later in the evening than the time when bees usually cease to labour, I concluded, that something more than ordinary was going forward. Going first to my own garden, and then to that of the cottager, I found a very considerable degree of bustle and agitation to prevail in one hive in each: every bee, as it arrived, seemed to be stopped

* Philos. Trans. for 1807, Part II, p. 234.

and

and questioned, at the mouth of each hive; but I could not discover any thing like actual resistance, or hostility, to take place; though I was much inclined to believe the intercourse between the hives to be hostile and predatory. The same kind of intercourse continued, in a greater or less degree, during eight succeeding days, and though I watched them very closely, nothing occurred to induce me to suppose, that their intercourse was not of an amicable kind. On the tenth morning, however, their friendship ended, as sudden and violent friendships often do, in a quarrel; and they fought most furiously; and after this there was no more visiting.

Ended in a quarrel.

Two years subsequent to this period I observed the same kind of intercourse to take place between two hives of my own bees, which were situate about two hundred yards distant from each other: they passed from each hive to the other just as they did in the preceding instance, and a similar degree of agitation was observable. In this instance, however, their friendfhip appeared to be of much shorter duration, for they fought most desperately on the fifth day; and then, as in the last mentioned case, all further visiting ceased.

Similar intercourse between two other hives.

Quarrelled on the fifth day.

I have some reason to believe, that the kind of intercourse I have described, which I have often seen, and which is by no means uncommon, not unfrequently ends in a junction of the two swarms; for one instance came under my observation, many years ago, in which the labouring bees, under circumstances perfectly similar to those I have described, wholly disappeared, leaving the drones in peaceable possession of the hive, but without any thing to live upon. I have also reasons for believing, that whenever a junction of two swarms, with their property, is agreed upon, that which proposes to remove, immediately, or soon afterward, unites with the other swarm, and returns to the deserted hive during the day only to carry off the honey: for having examined at night a hive from which I suspected the bees to be migrating, I found it without a single inhabitant. I was led to make the examination by information I had received from a very accurate observer, that all the bees would then be absent. A very considerable quantity of honey was in this in-

Sometimes two swarms form a junction.

stance

stance left in the hive without any guards to defend it; but I conclude, that the bees would have returned for it, had it remained till the next day. Whenever the bees quit their habitation in this way, I have always observed some fighting to take place; but I conceived it to be between the bees of the adjoining hives, and those which were removing; the former being attracted by the scent of the honey, which the latter were carrying off.

Bees settling in hollow trees, appear to send out scouts to examine them, after an individual has informed them of a proper place.

On the farm which I occupy, there were formerly many old decayed trees, the cavities of which were frequently occupied by swarms of bees; and when these were destroyed, a board was generally fitted to the aperture, which had been made to extract the honey; and the cavity was thus prepared for the reception of another swarm, in the succeeding season. Whenever a swarm came, I constantly observed, that about fourteen days previous to their arrival, a small number of bees, varying from twenty to fifty, were every day employed in examining, and apparently in keeping possession of the cavity; for if molested, they showed evident signs of displeasure, though they never employed their stings in defending their proposed habitation. Their examination was not confined to the cavity, but extended to the external parts of the tree above; and every dead knot particularly arrested their attention, as if they had been apprehensive of being injured by moisture, which this might admit into the cavity below; and they apparently did not leave any part of the bark near the cavity unexamined. A part of the colony, which purposed to emigrate, appeared in this case to have been delegated to search for a proper habitation; and the individual who succeeded must have apparently had some means of conveying information of his success to others; for it cannot be supposed, that fifty bees should each accidentally meet at, and fix upon, the same cavity, at a mile distant from their hive, which I have frequently observed them to do, in a wood where several trees were adapted for their reception; and indeed I observed, that they almost uniformly selected that cavity, which I thought best adapted to their use.

It not unfrequently happened, that swarms of my own bees took possession of these cavities, and such swarms were in

in several instances followed from my garden to the trees: and they were observed to deviate very little from the direct line between the one point and the other; which seems to indicate, that those bees, which had formerly acted as purveyors, now became guides.

Two instances came under my own observation, in which a swarm was received into a cavity, of which another swarm had previous possession. In the first instance I arrived with the swarm, and I could not discover, that the least opposition was made to their entrance: in the second instance, observing the direction that the swarm took, I used all the expedition I could to arrive first at the tree, to which I supposed they were going, whilst a servant followed them; and a descent of ground being in my favour, and the wind against them, I succeeded in arriving at the tree some seconds before them; and I am perfectly confident, that not the least resistance was opposed to their entrance.

Swarms admitted amicably into hollows already occupied.

Now it does not appear probable, that animals so much attached to their property as bees are, so jealous of all approach towards it, and so ready to sacrifice their lives in defence of it, should suffer a colony of strangers, with whose intentions they were unacquainted, to take possession, without making some effort to defend it: nor does it seem much more probable, that the same animals, which spent so much time in examining their future habitation, in the cases I have mentioned, should have attempted in this case to enter without knowing whether there was space sufficient to contain them, and without any examination at all. I must therefore infer, that some previous intercourse had taken place between the two swarms, and that those in the possession of the cavities were not unacquainted with the intentions of their guests; though the formation of any thing like an agreement between the different parties be scarcely consistent with the limitations generally supposed to be fixed by nature to the instinctive powers of the brute creation.

A previous communication between them must have taken place.

Brutes have evidently language; but it is a language of passion only, and not of ideas. They express to each other sentiments of love, of fear, and of anger; but they appear to be wholly incapable of transmitting to each other any ideas

Brutes have language to express passions only;

ideas they have received from the impression of external objects. They convey to other animals of their species, on the approach of an enemy, a sentiment of danger; but they appear wholly incapable of communicating what the enemy is, or the kind of danger apprehended. A language of more extensive use seems, from the preceding circumstances, to have been given to bees; and if it be not, in some degree, a language of ideas, it appears to be something very similar.

but bees must communicate ideas.

When a swarm of bees issues from the parent hive, they generally soon settle on some neighbouring bush or tree; and as in this situation they are generally not at all defended from rain or cold, it is often inferred, that they are less amply gifted with those instinctive powers, that direct to self-preservation, than many other animals. But their object in settling soon after they leave the hive is apparently nothing more than to collect their numbers; and they have generally, I believe always, another place to which they intend subsequently to go: and if the situation they select be not perfectly adapted to secure them from injuries, it is probably, in almost all instances, the best they can discover. For I have very often observed, that, when one of my hives was nearly ready to swarm, one of the hollow trees I have mentioned (and generally that best adapted for the accommodation of a swarm) was every day occupied by a small number of bees; but that after the swarm had issued from that hive, and had taken possession of another, the tree was wholly deserted; whence I inferred, that the swarm, which would have taken possession of the cavity of that tree, had relinquished their intended migration, when a hive was offered them at home, And I am much disposed to doubt, whether it be not rather habit, produced by domestication, during many successive generations, than any thing inherent in the nature of bees, which induces them to accept a hive, when offered them, in preference to the situation they have previously chosen: for I have noticed the disposition to migrate to exist in a much greater degree in some families of bees than in others; and the offspring of domesticated animals inherit, in a very remarkable manner, the acquired habits of their parents. In all animals this is observable; but

A colony of bees settles soon after quitting the hive.

This merely to collect the party together.

Choose the best place that offers,

and relinquish an intended settlement in a hollow tree, when a hive is offered them.

This preference arises from habit.

but in the dog it exists to a wonderful extent; and the offspring appears to inherit not only the passions and propensities, but even the resentments, of the family from which it springs. I ascertained by repeated experiment, that a terrier, whose parents had been in the habit of fighting with polecats, will instantly show every mark of anger, when he first perceives the scent of that animal; though the animal itself be wholly concealed from his sight. A young spaniel brought up with the terriers showed no marks whatever of emotion at the scent of the polecat; but it pursued a woodcock, the first time it saw one, with clamour and exultation: and a young pointer, which I am certain had never seen a partridge, stood trembling with anxiety, its eyes fixed, and its muscles rigid, when conducted into the midst of a covey of those birds. Yet each of these dogs is a mere variety of the same species; and to that species none of these habits are given by nature. The peculiarities of character can therefore be traced to no other source than the acquired habits of the parents, which are inherited by the offspring, and become what I shall call instinctive hereditary propensities. These propensities, or modifications of the natural instinctive powers of animals, are capable of endless variation and change; and hence their habits soon become adapted to different countries and different states of domestication, the acquired habits of the parents being transferred hereditarily to the offspring. Bees, like other animals, are probably susceptible of these changes of habit, and thence, when accustomed through many generations to the hive, in a country which does not afford hollow trees, or other habitations adapted to their purpose, they may become more dependent on man, and rely on his care wholly for a habitation; but in situations where the cavities of trees present to them the means of providing for themselves, I have found, that they will discover such trees in the closest recesses of the woods, and at an extraordinary distance from their hives; and that they will keep possession of such cavities in the manner I have stated: and I am confident that, under such circumstances, a swarm never issues from the parent hive, without having previously selected some such place to retire to.

Remarkable effects of hereditary habit in dogs.

These habits altered and modified by circumstances.

Bees never migrate till they have selected a habitation.

It

Bees not only carry farina on their thighs, but other matters.

It has been remarked by Mr. John Hunter, that the matter which bees carry on their thighs is the farina of plants, with which they feed their young, and not the substance with which they make their combs; and his statement is, I believe, perfectly correct: but I have observed, that they will also carry other things on their thighs. I frequently covered the decorticated parts of trees, on which I was making experiments, with a cement composed of bees-wax and turpentine; and in the autumn I have frequently observed a great number of bees employed in carrying off this substance. They detached it from the tree with their forceps, and the little portion thus obtained was then transferred by the first to the second leg, by which it was deposited on the thigh of the third: the farina of plants is collected and transferred in the same manner. This mixture of wax and turpentine did not, however, appear to have been employed in the formation of combs; but only to attach the hive to the board on which it was placed, and probably to exclude other insects, and air during winter. Whilst the bees were employed in the collection of this substance, I had many opportunities of observing the peaceful and patient disposition of them as individuals, which Mr. Hunter has also, in some measure noticed. When one bee had collected its load, and was just prepared to take flight, another often came behind it, and despoiled it of all it had collected. A second, and even a third, load was collected and lost in the same manner, and still the patient insect pursued its labour, without betraying any symptoms of impatience or resentment. When, however, the hive is approached, the bee appears often to be the most irritable of all animals; but a circumstance I have observed amongst another species of insects, whose habits are in many respects similar to those of bees, induces me to believe, that the readiness of the bees, to attack those who approach their hives, does not in any degree spring either from the sense of injury or apprehensions of the individual, who makes the attack. If a nest of wasps be approached without alarming its inhabitants, and all communication be suddenly cut off between those out of the nest, and those within it, no provocation will induce the former to defend their nest, or themselves. But

A compound of wax and turpentine taken thus by them,

and used as a cement.

The bee very patient as an individual.

Wasps similar in their habits.

But if one escape from within, it comes with a very different temper, and appears commissioned to avenge public wrongs, and prepared to sacrifice its life in the execution of its orders. I discovered the circumstance, that wasps thus excluded from their nest would neither defend it nor themselves, at a very early period of my life; and I profited so often, by the discovery, as a schoolboy, that I am quite certain of the fact I state; and I do not entertain any doubt, though I speak from experiments less accurately made, that the actions of bees, under similar circumstances, would be the same*.

Not so when apparently sent out to fight.

Mr. Hunter conceived bees wax to be an animal substance, which exuded between the scales of the belly of the insect; but I am strongly disposed to believe, that it is collected from plants, and merely deposited between the scales of the belly of the bee, for the joint purposes of being carried with convenience, and giving it the temperature necessary for being moulded into combs: and I am led to this conclusion, not only by the circumstance of wax being found in the vegetable world, but also by having often observed bees employed in detaching something from the bases of the leaves of plants with their forceps, which they did not deposit on their thighs,

Mr. Hunter mistaken in supposing bees wax an animal substance.

* A curious circumstance, relative to wasps, attracted the notice of some of my friends last year, and has not, I believe, been satisfactorily accounted for. A greater number of female wasps were observed in different parts of the kingdom, in the spring and early part of the summer of that year, than at almost any former period; yet scarcely any nests, or labouring wasps, were seen in the following autumn; the cause of which I believe I can explain. Attending to some peach trees in my garden, late in the autumn of the year 1805, on which I had been making experiments, I noticed, during many successive days, a vast number of female wasps, which appeared to have been attracted there by the shelter and warmth of a south wall; but I did not observe any males. At length, during a warm gleam in the middle of one of the days, a single male appeared, and selected a female close to me; and this was the only male I saw in that season. The male wasp, which is readily distinguishable from the female and labourer, by his long antennæ and shining wings, and by a blacker and more slender body, is rarely seen out of the nest, except in very warm days, like the drone bee; and the nests of wasps, though very abundant in the year 1805, were not formed till remarkably late in the season; and thence I conclude, that the males had not acquired maturity till the weather had ceased to be warm, and that the females, in consequence, retired to their long winter sleep without having had any intercourse with them.

Abundance of female wasps in 1786.

This accounted for.

as they do (I believe invariably) the farina of plants. I have also frequently observed the combs of very late swarms to be remarkably thin, and white, and brittle; which are circumstances very favourable to the conclusion, that the wax is a vegetable substance, for it would probably be less abundant during autumn than in summer; and that portion which had remained on the plants till late in the season would hence become more colourless by exposure to light, as well as more dry and brittle than when it first exuded; but were it an animal substance, there does not appear any reason, why it should be more dry and brittle, or less abundant, in the autumn, than in the spring and summer. The conclusions of Mr. Hunter are, however, always drawn with so much caution, and he united so much skill and science with the greatest degree of industry, that it is not without much hesitation and diffidence, that I venture to put my opinion in opposition to his authority.

Elton, May 4, 1807. T. A. KNIGHT.

III.

Description of a Mercurial Pendulum. Communicated by Mr. BARRAUD, *of Cornhill, who has made several, and has been highly satisfied with their performance in the Measure of Time.*

Description of a mercurial pendulum.

THE whole length of the pendulum rod, from the rivet that joins the spring to its top, to the end of the screw at L, fig. 1, Pl. VII, is $33\frac{8}{10}$ inches, (say 34 inches). The side pieces of the frame M M are of steel, as thick as the rod, that is $\frac{1}{4}$ of an inch, and not less. The top of the frame H consists of two pieces of steel, each $\frac{1}{8}$ of an inch thick, shaped as in the drawing, and screwed over the ends of the side pieces M M. The inside height of the frame, from E to A, is $8\frac{3}{10}$ inches, and the inside width between the pieces M M about $2\frac{3}{4}$ inches, so that the cylinder stands $\frac{1}{4}$ of an inch clear of them. The bottom piece N is $\frac{1}{2}$ an inch thick from

M. Ziarrandi's
Mercurial
Pendulum

Fig. 1.

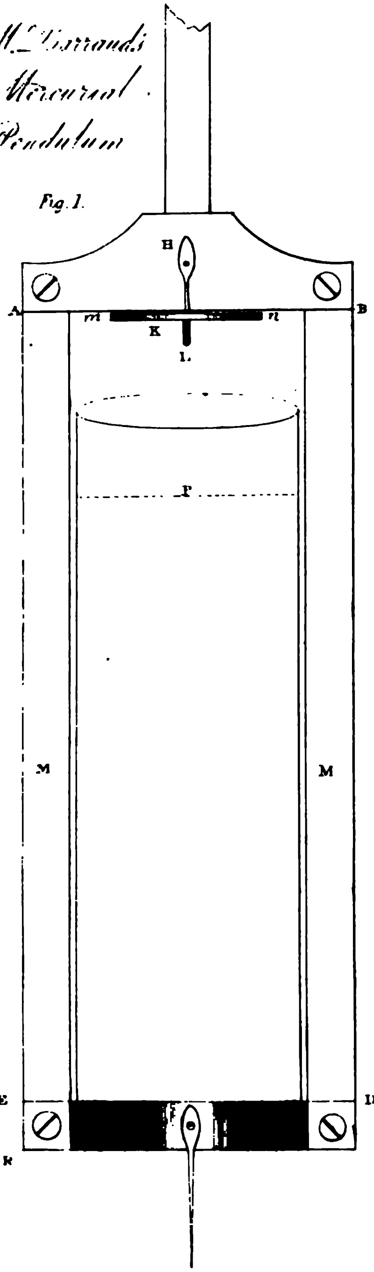
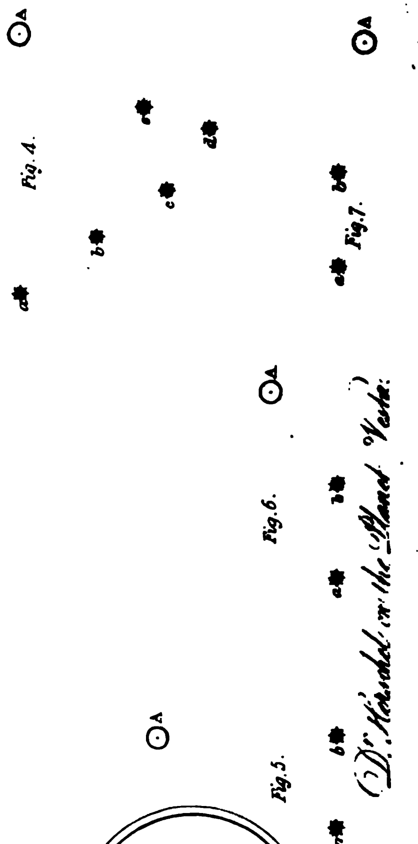


Fig. 4.

Fig. 5.

Fig. 6.

Fig. 7.



Dr. Herschel on the Planet Vesta.

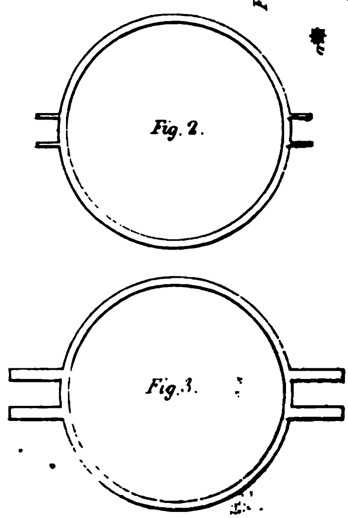


Fig. 2.

Fig. 3.

from E to R, and hollowed down to $\frac{1}{8}$ of an inch, so as to fit the bottom of the cylinder.

Description of a mercurial pendulum.

L is the bottom of the rod, and one inch of the end of it is made into a screw, that has forty threads in an inch. The nut K is $\frac{3}{4}$ of an inch deep, and the diameter of its circle from *m* to *n* is $1\frac{6}{10}$ inch, having the upper edge divided into 28 equal parts, and figured 0, 1, 2, 3, or at each 7th division. Each of these divisions is very nearly equal to 1″ in 24 hours.

The quantity of quicksilver required is between 10 and 11 lbs. It should fill the glass cylinder up to P, being 6·4 inches from the bottom of the glass, measured internally. Fig. 2 is the cover of the glass cylinder, and fig. 3 the bottom of the frame, that supports the cylinder, both viewed vertically.

If with this pendulum the clock be found to go right with the thermometer at 30°, and loses 1″ in 24 hours with the thermometer at 90°, it will be remedied by adding 10 oz. of quicksilver; and if the reverse by taking out that quantity.

The rod should be $\frac{1}{4}$ of an inch thick, and $\frac{3}{8}$ of an inch wide. The spring should be an inch long, and pretty stiff.

IV.

Observations on the Nature of the new celestial Body discovered by Dr. OLBERS, *and of the Comet which was expected to appear last January in its return from the Sun. By* WILLIAM HERSCHEL, *L. L. D. F. R. S.**

THE late discovery of an additional body belonging to the solar system by Dr. Olbers having been communicated to me the 20th of April, an event of such consequence engaged my immediate attention. In the evening of the same day I tried to discover its situation by the information I had obtained of its motion; but the brightness of the moon, which was near the full, and at no great distance from the object for which I looked, would not permit a star of even the 5th magnitude to be seen; and it was not till the 24th, that a tolerable view

Account of the new planet received April 20th, 1807.

* Philos. Trans. for 1807, P. II, p. 260.

could

could be obtained of that space of the heavens, in which our new wanderer was pursuing its hitherto unknown path.

Looked for. As soon as I found that small stars might be perceived, I made several delineations of certain telescopic constellations, the first of which was as represented in fig. 4, Pl. VII, and I fixed upon the star *A*, as most likely, from its expected situation and brightness, to be the one I was looking for. The stars in this figure, as well as in all the other delineations I had made, were carefully examined with several magnifying powers, that in case any one of them should hereafter appear to have been the lately discovered object, I might not lose the opportunity of an early acquaintance with its condition. An observation of the star marked *A*, in particular, was made with a very distinct magnifying power of 460, and says, that it had nothing in its appearance that differed from what we see in other stars of the same size; indeed Dr. Olbers, by mentioning in the communication which I received, that with such magnifying powers as he could use, it was not to be distinguished from a fixed star*, had already prepared me to expect the newly discovered heavenly body to be a valuable addition to our increasing catalogue of asteroids.

Presumed to be an asteroid.

The 25th of April I looked over my delineations of the preceding evening, and found no material difference in the situation of the stars I had marked for examination; and in addition to them new asterisms were prepared, but on account of the retarded motion of the new star, which was drawing towards a period of its retrogradation, the small change of its situation was not sufficiently marked, to be readily perceived the next day when these asterisms were again examined, which it is well known can only be done with night-glasses of a very low magnifying power.

A long interruption of bad weather would not permit any regular examination of the situation of small stars; and it was only when I had obtained a more precise information from the Astronomer Royal, who, by means of fixed instru-

* Der neue planet zeigt sich als ein stern zwischen der 5ten und 6ten grösse, und ist im fernrohr, wenigsten mit den vergrösserungen die ich anwenden kann, von einen fixstern nicht zu unterscheiden.

ments,

ments, was already in possession of the place and rate of motion of the new star, that I could direct my telescope with greater accuracy by an application of higher magnifying powers. My observations on the nature of this second new star discovered by Dr. Olbers are as follow.

April 24. This day, as we have already seen, the new celestial object was examined with a high power; and since a magnifier of 460 would not show it to be different from the stars of an equal apparent brightness; its diameter must be extremely small, and we may reasonably expect it to be an asteroid. Observations of it.

May 21. With a double eye-piece magnifying only 75 times, the supposed asteroid A makes a right-angled triangle with two small stars *a b*. See fig. 5.

With a very distinct magnifier of 460 there is no appearance of any planetary disk.

May 22. The new star has moved away from *a b*, and is now situated as in fig. 6. The star A of fig. 4 is no longer in the place where I observed it the 24th of April, and was therefore the asteroid. I examined it now with gradually increased magnifying powers, and the air being remarkably clear, I saw it very distinctly with 460, 577, and 636. On comparing its appearance with these powers alternately to that of equal stars, among which was the 463d of Bode's Catalogue of the stars in the Lion of the 7th magnitude, I could not find any difference in the visible size of their disks.

By the estimations of the distances of double stars, contained in the first and second classes of the catalogues I have given of them, it will be seen, that I have always considered every star as having a visible, though spurious, disk or diameter; and in a late paper I have entered at large into the method of detecting real disks from spurious ones; it may therefore be supposed that I proceeded now with Vesta (which name I understand Dr. Olbers has given the asteroid), as I did before in the investigation of the magnitudes of Ceres, Pallas, and Juno.

The same telescopes, the same comparative views, by which the smallness of the latter three had been proved, Similar to Ceres, Pallas, and Juno.

convinced

convinced me now, that I had before me a similar fourth celestial body.

Described. The disk of the asteroid which I saw was clear, well defined, and free from nebulosity. At the first view I was inclined to believe it a real one; and the Georgian planet being conveniently situate, so that a telescope might without loss of time be turned alternately either to this or to the asteroid, I found that the disk of the latter, if it were real, would be about one sixth of the former, when viewed with a magnifying power of 460. The spurious nature of the asteroidal disk, however, was soon manifested by an increase of the magnifying power, which would not proportionally increase its diameter as it increased that of the planet; and a real disk of the asteroid still remains unseen with a power of 636.

Farther observations. May 23. The new star has advanced, and its motion is direct; its situation with respect to the two small stars *a b*, is given in fig. 7.

Its apparent disk with a magnifier of 460 is about 5 or 6 tenths of a second; but this is evidently a spurious appearance, because higher powers destroy the proportion it bears to a real disk when equally magnified. The air is not sufficiently pure this evening to use large telescopes.

May 24. With a magnifying power of 577 I compared the appearance of the Georgian planet to that of the asteroid, and with this power the diameter of the visible disk of the latter was about one 9th or 10th part of the former. The apparent disk of the small star near β Leonis, which has been mentioned before, had an equal comparative magnitude, and probably the disks of the asteroid and of the star it resembles are equally spurious.

The 20 feet reflector, with many different magnifying powers, gave still the same result; and being already convinced of the impossibility, in the present situation of the asteroid, which is above two months past the opposition, to obtain a better view of its diameter, I used this instrument chiefly to ascertain, whether any nebulosity or atmosphere might be seen about it. For this purpose the valuable quantity of light collected by an aperture of 18¾ inches directly received by an eye-glass of the front view without a second reflection,

reflection, proved of eminent use, and gave me the diameter of this asteroid intirely free from all nebulous or atmospheric appearances.

The result of these observations is, that we now are in possession of a formerly unknown species of celestial bodies, which, by their smallness and considerable deviation from the path in which the planets move, are in no danger of disturbing, or being disturbed by them; and the great success that has already attended the pursuit of the celebrated discoverers of Ceres, Pallas, Juno, and Vesta, will induce us to hope, that some further light may soon be thrown upon this new and most interesting branch of astronomy.

Observations of the expected Comet.

The comet which has been seen descending to the sun, and from the motion of which it was concluded, that we should probably see it again on its return from the perihelion, was expected to make its reappearance about the middle of last January, near the southern parts of the constellation of the whale. **Reappearance of a comet expected.**

January 27. Towards the evening, on my return from Bath, where I had been a few days, I gave my sister Carolina the place where this comet might be looked for, and between flying clouds, the same evening about $6^h\ 49'$, she saw it just long enough to make a short sketch of its situation. **Seen by Miss Herschel.**

January 31. Clouds having obscured the sky till this time, I obtained a transitory view of the comet, and perceived that it was within a few degrees of the place which had been assigned to it; the unfavourable state of the atmosphere, however, would not permit the use of any instrument proper for examining it minutely.

There will be no occasion for my giving a more particular account of its place, than that it was very near the electrometer of the constellation, which in Mr. Bode's maps is called *machina electrica*; the only intention I had in looking for it being to make a few observations upon its physical condition.

February 1. The comet had moved but very little from the place where it was last night; and as the air was pretty clear, **Described.**

clear, I used a 10-feet reflector with a low power to examine it. There was no visible nucleus, nor did the light which is called the coma increase suddenly towards the centre, but was of an irregular round form, and with this low power extended to about 5, 6, or 7 minutes in diameter. When I magnified 169 times it was considerably reduced in size, which plainly indicated, that a farther increase of magnifying power would be of no service for discovering a nucleus. On account of cloudy weather I never had an opportunity of seeing the comet afterwards.

Compared with others.

When I compare these observations with my former ones of 15 other telescopic comets, I find, that, out of the 16 which I have examined, 14 have been without any visible solid body in their centre, and that the other two had a very ill defined small central light, which perhaps might be called a nucleus, but did not deserve the name of a disk.

V.

Observations and Measurements of the Planet Vesta. By JOHN JEROME SCHROETER, *F.R.S.**

Planet Vesta has no disk with a power of 300, and an intense radiating light, like a star of the 6th magnitude.

AT our very first observations with magnifying powers of 150 and 300 applied to the excellent new 15-feet reflector, we found the planet Vesta *without any appearance of a disk*, merely as a point like a fixed star with an intense, radiating light, and exactly of the same appearance as that of any fixed star of the sixth magnitude. In the same manner we both afterwards saw this planet several times with our naked eyes, when the sky was clear, and when it was surrounded by smaller invisible stars, which precluded all possibility of mistaking it for another. This proves how very like the intense light of this planet is to that of a fixed star.

The same with other telescopes.

As the observations and measurements of Ceres, Pallas, and Juno, were made with the same eye-glasses, but with the 13-feet reflector, we soon after compared the planet Vesta with the same glasses of 136 and 288 times magnifying

* From the Philos. Trans. for 1807, Part II, p. 245.

ing power in the 13-feet reflector. In both these telescopes its image was, *without the least difference*, that of a fixed star of the 6th magnitude with an intense radiating light; so that this new planet may with the greatest propriety be called an *asteroid*.

An asteroid.

April 26th in the evening at 9 o'clock, true time, I succeeded in effecting the measurement of Vesta, with the same power of 288, by means of the 13-feet reflector, with which that of Ceres, Pallas, and Juno had been made; and when viewed by this reflector it also appeared exactly in the same manner. Of several illuminated disks, of 2·0 to 0·5 decimal lines, which I had before made use of for measuring the satellites of Saturn and Jupiter, the smallest disk only of 0·5 lines could be used for this purpose; by it the rounded nucleus of the planet Vesta, when the disk was at the distance of 611·0 lines from the eye, appeared *at most* of the same size, and I must even estimate its diameter as $\frac{1}{6}$ smaller. If therefore, we attend, not to the full magnitude of the projection, but the estimation just mentioned, it follows by calculation that the *apparent diameter of the planet Vesta* is *only* 0·488 *of a second*, and consequently only *half* of what I have found to be the apparent diameter *of the fourth satellite of Saturn*.

Measured.

Its apparent diameter only 0·488″, or half that of the 4th satellite of Saturn.

This extraordinary smallness, with such an intense, radiant and unsteady light of a fixed star, is the more remarkable, as, according to the preliminary calculations of Dr. Gauss, there can be no doubt that this planet is found in the same region between Mars and Jupiter, in which Ceres, Pallas, and Juno, perform their revolutions round the sun; that, in close union with them, it has the same cosmological origin; and that as a planet of such smallness and of so very intense light, it is comparatively *near to the earth*. This remarkable circumstance will no doubt be productive of important cosmological observations, as soon as the elements of the new planet have been sufficiently determined, and its distance from the Earth ascertained by calculation.

It is between Mars and Jupiter.

Lilienthal, May 12, 1807.

VI.

VI.

On a new Method of Slating, and constructing the Roofs of Houses: by Mr. Lewin Tugwell *.

Principles of Mr. Tugwell's method of roofing.

THE leading principles of Mr. Tugwell's plan are, to save slate and timber, thus diminishing the expense of a roof; and at the same time to render it secure against the admission of wind or water. The saving of slate is effected by lowering the pitch of the roof. This likewise diminishes the length of the rafters, which at the same time are placed farther asunder than usual; and besides this the boarding, usually placed under slates to keep out the wet, is dispensed with entirely. An additional advantage he observes is connected with his roof. It possesses such superior strength, as to be capable of sustaining, if necessary, partitions, and floors connected with them, even down to the ceilings of the modern enlarged dining rooms, if they be appropriately constructed and suspended from it; thus superseding the necessity of the otherwise expensive and complicated construction of spliced beams, ceiling joists, &c., saving timber and workmanship in these also; and finally, by thus combining in a frame the roof, partitions, and floors of a building, of rendering the whole much more firm and compact, than any mode hitherto used.

His new mode described.

The peculiarities of the mode, and as such necessary to be pointed out, cannot be described, and consist in,

1st. A diminution in its elevation, seen in the beam-rafters A A, Pl. VIII, fig. 1, giving an angle of only twelve degrees from the horizon, whereby both its timbers and slates will be lessened in quantity in a ratio generally as of three to four.

2dly. The increased distance of these rafters, as at B B, fig. 2, one from another, *i. e.* to two feet. And as, in the modes hitherto used, they are generally at not more than 15 inches asunder, a farther saving will therein be found of

* Abridged from the Letters and Papers of the Bath and West of England Society, vol. XI.

more

more than one in three, or as in the proportion of 18 to 11; and which, together with the diminution in their length above-mentioned, while they combine in a system of far greater strength and duration, will incur a saving in the slating as aforesaid of about a fourth part, and in the raftering of considerably more than half.

New mode of slating described.

C C, figs. 1, 2, Wall-plates in substance considerably increased; viz. to six inches square.

D D D, figs. 1, 2, Foot-beams, firmly inserted in the wall-plates, by means of dove-tailed joints, at six feet distance from each other; one of which joints is seen at fig. 8, laid open, to display the operation of the wedge; as, should it inadvertently be driven in on the inside of the plate, and where floors, partitions, &c. as before hinted, on any occasion, are to be suspended on the roof-timbers, it would necessarily draw, and derange the whole of the superstructure*.

E E E, figs. 1, 2, First-piece, of peculiar shape and strength, being two inches thick, and *nine* deep.

F F, figs. 1, 2, Purloins, or side-pieces, let in, and spike-nailed to the queen-posts, at right angles with the rafters, and at equal distances from their extremities.

G G, fig. 2, Plate-rafters, let into the wall-plates at their lower ends, screwed at their centres to the purloins, and firmly fastened by an appropriate joint (see fig. 3) to the first pieces. Thus in these is more distinctly seen the peculiar and singular stability of the system. As each of the foot-beams, together with its sustained and sustaining rafters, king and queen posts, &c., forms an arch, or rather a series of arches, of such permanency as not to be subdued, while their parts remain uncrushed, the wall-plates C C, fastened

* Should it at any time be foreseen that a more than ordinary weight will be found in floors, partitions, &c., thus suspended on the roof-timbers, it will be only necessary to enlarge the size of the latter, and they may therein be adapted to any scale required. If, however, there should be a probability that an alteration in the upper chambers may at some future period take place, and wherein a removal of the partitions may become necessary, although it would be far from being impracticable, the method may, notwithstanding, all things considered, perhaps not be found the most eligible.

to

New mode of slating described.

to them by dove-tailed joints as aforesaid, constitute, for these intervenient rafters, abutments as immovable as those of the beam-rafters themselves; and which, like those, being firmly fastened to the purloins F F, at their centres, and to the first-pieces at their upper ends; while these first-pieces remain unbent in an upward direction, and the wall-plates are found immovable outwards, they form, each pair of them, as permanent an arch as the beam-rafters themselves; and thus aiding the latter, they altogether constitute, as aforesaid, a frame of such singular and uniform strength and stability, as undoubtedly to be capable of sustaining at least any weight that it may ever be necessary to lay on it.

H H H, figs. 1, 2, Deal laths, each an inch thick, and two inches wide, their lower half rabbited for receiving the upper ends of the slates, in depth equal to the thickness of the latter.

I I, figs. 1, 2, Slates, nailed nearly at their centres to the upper parts of the laths, the nails clenched, and the slates cemented on both sides to each other with putty, or any other matter proper for uniting them; and thereby effectually excluding rain, wind, driving snow, and all aërial humidity.

K K, fig. 2, Ceiling-joists inserted in the foot-beams after the usual manner.

L, fig. 2, A portable stage or scaffold for the slater to work on between the rafters, for keeping at all times under his thumb a new and appropriate set of simple implements, (as usefully employed by rational beings in all other matters) and occasionally, as the work proceeds, to be drawn backward on the ceiling-joists.

M, fig. 1, The slater seated between the rafters on his stage, with his work before him, and immediately under his eye.

N, fig. 7, A discharging saw, that, being of proper temper, and having a series of teeth about three inches down on each side from its point, is occasionally introduced by a hammer at its heel, and thus removes putty, nails, &c. from a broken slate; when a new one, supplying its place, will, with a little putty under its lower edge to cement it, become quite as effectual, if not as firm, as the original one. Probably,

bably, the putty having been thus removed by the teeth of the saw up to the nail, a similar instrument, with however finer teeth, would more properly apply for cutting off the latter, when the first might again proceed to the entire separation of the slate. New mode of slating described.

O, fig. 4, A pallet of thin permanent board, that, being put into a mould for the purpose, receives on its upper surface putty, mortar, or any other proper cement; and which, being spread over it with a moistened wooden spatula, or striker, obtains a uniform thickness (a quarter of an inch more or less), governed by the depth of the mould, in the manner of forming bricks: this is afterward divided into half-inch slivers, and applied on the joints of the slates, as seen at P, fig. 2.

Q, fig. 5, The mould, of the same breadth on the inside as the pallet; and which, having an edge rising on three of its sides more than the thickness of the pallet, gives that of the cake of cement.

R, fig. 6, A two-edged knife, for dividing and applying the above slivers of mortar, putty, &c.

S, figs. 1 and 2, a small set or head of iron, about three pounds weight, to be taken with the left hand, at the driving of the nails, and pressed hard against their points, thus giving them an effectual clinching, and at the same time receiving the impulse of the hammer, so as to prevent all jarring, disjunction, and derangement. Still farther to guard against this, and to avoid the absurd practice of splitting the laths with almost every nail that is driven through them, an appropriate brad-awl, T, fig. 2, with a T head and chissel point, to cut its way across the grain, should be used to make a passage for the nail, previous to driving it.

While the pressure of every kind of slating on the timbers of a roof is found in all its parts equal, and the power of such timbers for sustaining it is, by the modes hitherto used, very partially applied, and chiefly found in those that have their lower ends set in its foot-beams; an idea of a peculiar uniformity and consequent stability in the general system here recommended, as well as separately in that of fastening down its slates, will, I humbly conceive, impress itself on every mind open to conviction. General defect of roofs.

Was

Slates should be cut at the quarries.

Was the value of these slates duly estimated, howsoever plenteous and inexhaustible at their quarries, they might there, by means of various patterns, saws, drills, rasps, and other proper machinery, while moist and soft, be formed into differently sized parallelograms, with the greatest facility, accuracy, and dispatch; and every slate being made thereby to retain the utmost regular size its rough dimensions would admit of, much unnecessary waste would be avoided, and being afterwards regularly classed and denominated by the number of inches in their lengths and breadths respectively, (as nine fifteens, ten eighteens, &c. instead of the burlesque terms of *ladies*, *countesses*, and *duchesses)* they might with much less expense be conveyed to their respective destinations; and when, whatever the class preferred, they would be also much more conveniently and effectively applied, than if of various shapes and sizes. Mill-stones, grindstones, and indeed all others, if raised at a distance from their respective destinations, are prudently divested of all superfluous matter and weight at their quarries; and, but for its claim to exemption from all that is rational, there is no cause why the same economy may not be used in the removal of slate.

Thatching should be discarded.

From considerations of the great scarcity, and high price of timber in general, and consequent necessity for our regarding the most frugal use of that article; also, the immense waste of that ground-work of all our wealth and support, manure for our lands, that has, through all ages, from time immemorial prevailed in the use of thatch; and finally, from the certain and very great danger of the latter being destroyed by fire; the obvious absurdity of using it at all, wherever a better material may be obtained, one might naturally suppose would evince the propriety of an almost universal recourse to light thin slates, as a most eligible material for roofing in general.

Danger of fires instanced.

I recollect no less than six fires having taken place in my native village, from its cottages having been covered with *thatch*. One of them was occasioned by sparks from a forge; another by those from an oven; and three, if not four, were generally supposed to have proceeded from the hands of incendiaries. It is observable, that during the whole

whole period of the above only one fire has happened there under a roof formed of slate: and that in the building of all houses since, that article has judiciously obtained a preference.

With a view to general reformation in the matter, we may observe, that from the universal predilection, during about thirty years past, for that very beautiful and quick growing plant, the Lombardy poplar, wisely fostered in all crowded places, and particularly the metropolis, among other good purposes, for the purification of the atmosphere, its fine straight timbers begin now necessarily to be taken down and brought to market, so that in a short time we may expect an abundant supply; and although, being of a very light and soft texture, no particular use has yet been assigned them, there cannot be a doubt of their being ere long very generally used, at least for inferior buildings; the precautions being regarded of felling them always in winter, and when sawn, washing out their saccharine juices, by laying their scantlings awhile under water; and also giving them (together with their plank, boards, &c.) extra size, in proportion to their want of density. The Scotch fir likewise, from the scarcity and dearth of all other timber, and particularly foreign deals, begins now to be universally employed. And were the genius and peculiar properties of our immense tracts of waste lands thought worth attending to, it cannot be supposed, but that many of them, composed of light, pervious, blowing sands, and fit for little else, (such as about Basingstoke in Hampshire, and indeed to be found elsewhere in too many parts of the kingdom) might be rendered abundantly productive of this article; and which also, when felled and sawn, being properly washed, would be found very generally useful in better erections. Whenever their long horizontal roots may without obstruction extend themselves, howsoever infertile, in the common acceptation, the soil, their growth is generally more rapid than in land more rich; but at the same time more close and impervious*. Nor, it is to be hoped, will the idea be thought

Lombardy poplar.

Cautions respecting it.

Scotch firs.

* There are now lying on the Quay in this town, brought down by the Kennet and Avon Canal, many fine trees of larch, with others of Scotch

thought visionary, that were a sufficiency of these articles thus easily and quickly procured, they might afterwards, by means of our already numerous and constantly increasing canals, and other improved modes of conveyance, together with a proper accompaniment of blue slate, or at least the factitious red pantiles, be transmitted to every town and village in the kingdom; whence the produce of perhaps richer lands might be remitted in return, and in some degree commensurate to the expense.

Much also might be done by appropriate and judicious planning; some houses containing by far more room, and particularly *useful* room, than others under the same or a less quantity of roofing.

Instance in proof of the adequacy of the method.

The danger of the slates being broken; and the insufficiency of the putty or cement, to keep the joints weathertight, have been objected to Mr. Tugwell's plan. In answer to this he points out a house thus covered in upward of three years ago, which has remained during that time impervious to wind, wet, or dampness of any kind from the air.

VII.

Heights of various Places in France, &c.; by Dr. BERGER. *Concluded from Vol. XVIII, p. 308.*

SECT. IV.

Brief description of some mountains in the department of Mont-Blanc.

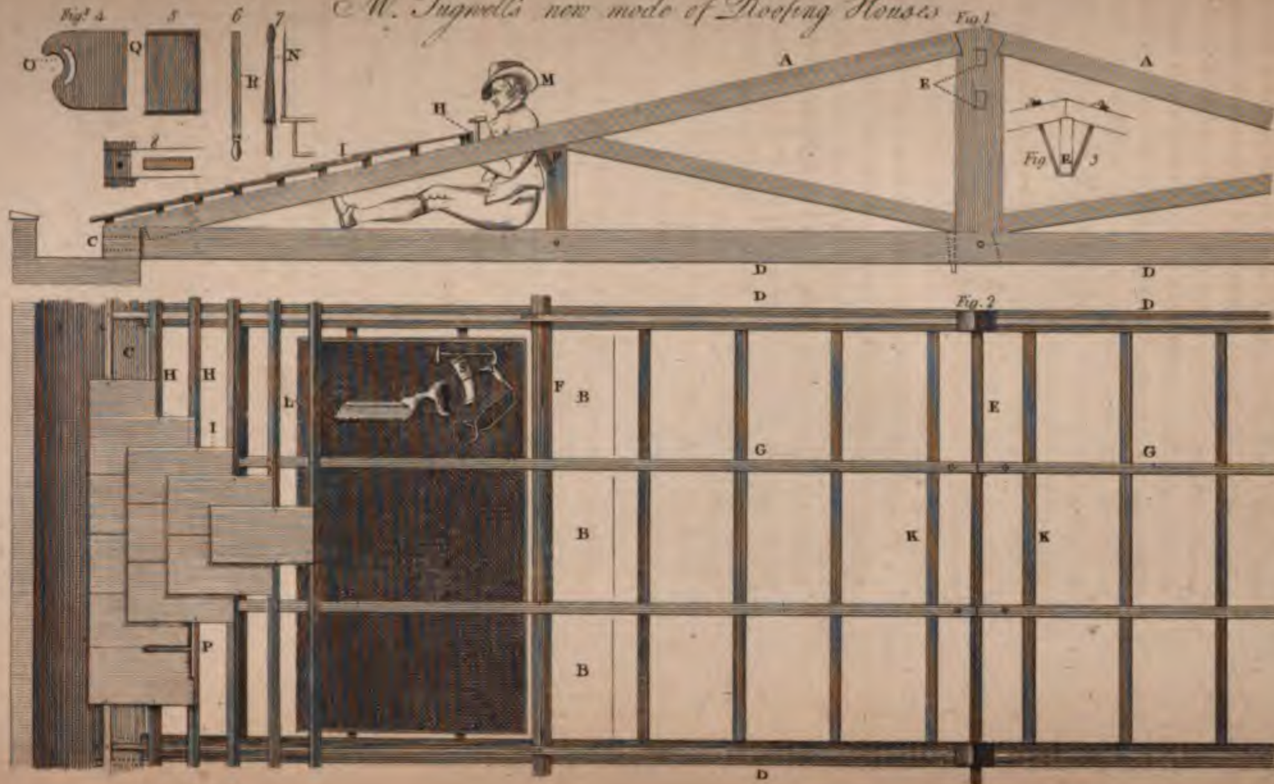
Valley des Bornes.

THE valley des Bornes, the bottom of which is scarcely higher than the plain of the lake of Geneva, and which is separated

Scotch and spruce fir, more than forty feet in length, although of less than forty years growth; they, several of them, square two feet at bottom, and nearly one at the top; many of the larch, approaching nearer to parallelisms, are straight, and free from knots; and the lower lengths of even the Scotch fir cut very good board; while their tops serve well for coarse roof timbers; but as the knots in these dispose their scantlings to warp in drying, care should be taken to soak them immediately from the saw-pit; and in about six weeks after, judiciously to stack them from the pool, placing the most knotty always at the bottom of the pile, whereby much of such warping would generally be prevented.

Method of treating knotty plants.

Mr. Tugwell's new mode of Roofing Houses



separated from it only by the mountains Salève, Sion, and Vouache, has for its limits to the south a chain of mountains in the same line as mount Brison, and the direction of which is parallel to that of the central chain. The course of the Arve, and the low mountains that skirt the western shore of the lake of Annecy, are its boundaries to the north-east and south-west.

Mr. Saussure has described only the mountains on the north of this valley. The chain on the south, taken together, is at least 15 miles long. Its greatest height, like that of the Jura, is to the south-west. One mountain there, la Tournette, rises 940 toises above its base. The precipices of this chain look from the Alps, that is to say, the strata slope toward them. The limestone that composes them is compact, including great numbers of imbedded flints, and not unfrequently we meet with calcareous rocks, the tops of which are completely capped with silex. Southern chain. La Tournette. Silex interspersed in limestone.

Though the mountains that form this chain are all connected together, except that the continuity is occasionally interrupted by a transverse valley, they have almost as many different names, as there are parishes at their feet. The strata are much more regular in the north-east part of the chains, than in the south-west. Thus the mountains of St. Laurence display horizontal banks, much resembling those of mount Salève; while at Villaz and Dingy, where the principal branch of the chain has a perceptible inflexion to the south, the strata lose their uniform horizontality, and this more and more as we approach Tournette, where we see some completely broken, others arched or raised upon themselves; a character, as already observed, announcing the vicinity of a transition chain. The back of this chain toward the Alps falls into the valley of little Bornand, at the bottom of which flows the river Borne. Over this river is a bridge more than sixty feet high. Variation in the strata.

The two principal summits of this chain are Pormonaz and Tournette. Pormonaz is nearly in the middle of the chain, and rises 540 toises above its base. The first part of the ascent is through a very thick wood, from which, after a journey of two hours and half, you enter into rich pastures, bounded on all sides, except to the north-east, by Pormonaz. Thick wood. Pastures.

cliffs more than 180 toises high. In these meadows, which form a natural amphitheatre, are huts, where you may spend the night on occasion, and whence there is a pleasant view of part of lake Annecy and the surrounding country. From this station you gain the summit of the mountain in an hour and three quarters. Nothing can be more dreary than the top of mount Pormonaz. Figure to yourself a vast flat of limestone rock, perfectly bare and destitute of vegetation, intersected by clefts in every direction, like the table-land of mount Plattet, and you will have a just idea of it. Here and there at the bottom of these clefts are seen the dry trunks of some lifeless firs, the brown colour of which forms a striking contrast with the whiteness of the rocks amidst which they are found.

Extensive bare flat.

Flints.

In no part of the chain did I find nodules and caps of flints so abundant. The analysis of one of these nodules gave me 0·87 of siliceous earth: yet when reduced to powder an acid excited a slight effervescence in it, from a small quantity of calcareous earth, either forming a constituent part of it, or from which the surface could not be completely freed.

Tournette.

From the summit of Pormonaz Tournette appears to advantage. It is seen directly south, and exceeding in height most of the mountains around it as much as Mont Blanc surpasses the summits in its vicinity.

Ascent to it.

Tournette may be ascended by the way of Talloires, but more easily by that of Thônes, a small but very ancient town, now the chief place of a circle, which is seated at its east foot. This town is built in a very narrow valley, which on this account enjoys a warmer temperature than the larger vale, as is evident from the plants it produces.

Thônes.

From Thônes you proceed along the bottom of this valley for half an hour, then cross a branch of the Sière on a wooden bridge, and reach the hamlet of Clefs. Thence the road continues through a wood of beech and firs, not very thick, to about one third the height of the mountain. At this place are a few summer huts. The first time I ascended Tournette, which was in 1799, I found on the 12th of August, some distance above these huts, a cake of frozen snow, several feet thick at bottom, the only place where

Clefs.

Recent glacier.

where its thickness could be estimated, and extending up the acclivity of the mountain quite to its summit. The preceding winter having been severe, so much snow had fallen, that the heat of summer had not been able to melt it, a circumstance unheard of before, for the oldest inhabitants could not recollect, that the snow had ever remained longer than the end of June. I conceived this mass of frozen snow might prove the nucleus of a glacier of the second order*: and in fact the following year, when I returned thither in July, I found the snow, far from having diminished, had rather increased, so that the recent production of a glacier on this mountain appeared to me very probable.

On approaching the summit the stone assumes a fissile character, which it had not below; and we meet with dodecaedral calcareous spar finely crystallized, and several fragments of gritstone, of which there is a stratum 116 toises above the level of the sea. I did not perceive any nodules of silex, which are so common on mount Pormonaz. The loftiest point of Tournette is a very remarkable rock. It is nearly circular, 94 feet high, and 145 in diameter, standing alone on a point of the ridge that forms the summit, and cut perpendicularly nearly alike on every side. There is no getting to the top of it, but my means of steps cut in the rock on the north-east. It is no doubt from this rock, standing there like a sentry-box, and seen from all the surrounding country, that the mountain received its name. The prospect from it is very extensive and interesting. To the east it takes in the centre of the grand chain, and all the secondary ones attached to it in succession: to the south-east the mountains Tarenteuse and Maurienne: to the south-west those of the department of the Isère: the chain of Jura to the north-north-west: and the lake of Geneva to

Spar.

Grit.

No flint.

Singular rock.

Prospect from it.

* Mr. Saussure first distinguished the glaciers into two kinds. The first are included in the bottoms of the high valleys, almost all in a transverse direction, that terminate at bottom in the low longitudinal valleys, while at top they form grand *culs-de-sac* surrounded by inaccessible rocks. Such are those that terminate in the valley of Chamouni. Those of the second kind are not included in valleys, but spread over the slopes of lofty summits.

Glaciers of two kinds.

the

the north. Beneath your feet you have a bird's eye view of the lake of Annecy, and the fine plains around it: and westerly, toward the Lyonnois, the view extends very far, as there is nothing to interrupt it.

Another road. The road by way of Annecy and Talloires is much more laborious. The ascent from Talloires is very steep, and not free from danger. In the neighbourhood of that town is a fine vineyard, formerly belonging to a convent of Benedictines there; and the road to it from Menthon is shaded by walnut and chesnut trees. The mountain itself is very rich in plants, whichever way you ascend it.

Vineyard.

Chesnuts and walnuts.

The following table exhibits the heights of the different places that have been mentioned, in fathoms and thousandth parts above the level of the sea, as calculated from the height of the barometer, both according to the formula of Deluc and that of Trembley, with the mean temperature by Fahrenheit's thermometer, and the time when the observations were made.

The heights of some of the principal points, as given by Deluc, Pictet, and Saussure, are also added.

TABLE

TABLE of Heights above the Sea, in Toises and Thousandth Parts.

Department of the Leman Lake.

PLACES.	Time of the observation.	Mean Tem. by Fah.	Height acc. to the cal. of Deluc.	of Trembley.
Village of le Coin, foot of mount Salève	25 June, 1802, 5,30′ P. M.	69.7°	328·333	331·833
Summit of the little Salève*	21 June, 1801, 1 P. M.	67·8°	459·833	466·500
Barn of the 13 trees †	—— 10 A. M.	59·3°	591·833	601·500
Summit of mount Salève ‡	25 June, 1802, 2 P. M.	70·5°	697·166	710·666
Lussinge at the foot of mount Voirons	30 Sept. 1801, 7 A. M.	52·8°	346·166	368·500
South summit of Voirons	—— 1,50′ P. M.	63·3°	714·166	727·500
Northern summit, called Calvary §	—— 4,15′ P. M.	56·75°	741·333	752·166
Notre Dame d'Abondance	16 Sept. 1799, 8 A. M.	47·4°	443·833	449·333
Boundary cross between France and le Valais	—— 2 P. M.	65·8°	709·333	723·166
Huts of la Chiarre ‖ on the Môle	28 June, 1801, 2,15′ P. M.	71°	614·166	625·333

* According to Mr. Deluc, Rech. sur les Mod. de l'Atmos. 445 toises. † Ib. 601 toises. ‡ Ib. 700 toises.

§ According to Mr. Saussure, 707 toises. ‖ According to Pictet, 611·666.

Huts

PLACES.	Time of the observation.	Mean heat.	Accord. to Deluc.	Accord. to Trembley.
Huts of la Tour *	28 June, 1801, 8 A. M.	60·5°	730·363	743·666
——— Aisle †	9 Aug. 1799, 5 A. M.	64·1°	769.333	784·000
Summit of the mountain ‡	——— 11 A. M.	67·2°	937·833	957·666
	28 June, 1801, 10,30′ A.M.	61·6°	943·666	962·666
	——— noon	61·7°	941·833	961·833
Mean of the three obs.		....	941·110	960·386
Bonneville §	8 Aug. 1799, noon	81°	225·166	225·833
Samoin, in the vale of Taninge	24 July, 1801, noon	71·1°	352·333	356·166
Sixt ‖	——— 7,30′ P. M.	71°	385·166	386·333
	25 July, 1801, 12,15′	65·3°	383·833	380·500
Mean of the two obs.		....	384·499	387·416
Mount Brison ¶	24 July, 1800, 7 P. M.	62·9°	940·833	972·833
Defile of Encrenaz, on mount Vergi	16 Aug. 1799, 11 A. M.	64°	1019·000	1040·500
Lake Beni, or Saxonnex	17 Aug. 1799, 1 P. M.	62·3°	729·333	743·000
Another defile of mount Vergi	25 July, 1800, 2 P. M.	73·6°	1180·833	1208·333

* According to Saussure, 717·666.

† Deluc, 780·666. Pictet, 765·666.

‡ Deluc, 947·666 Pictet, 940·333.

§ Saussure, 226·666.

‖ Deluc, 25 Aug. 1765, 374·666: and 24, 25, 26 Aug. 1770, 374·833.

¶ Pictet, 943.

Lake

PLACES.	Time of the observation.	Mean heat.	Accord. to Deluc.	Accord. to Trembley.
Lake Lessy	15 Aug. 1799, 6 A. M.	61·1°	865·500	884·166
Chartreuse of Reposoir	——— 5 P. M.	69·4°	530·833	539·833
	26 July, 1800, 9 A. M.	65°	526·500	535·166
	22 July, 1801, 5 A. M.	53·6°	523·500	531·333
Mean of the three obs.		....	526·944	535·444
Summit of Point-de-Château	26 July, 1800, 4 P. M.	65·5°	1258·833	1296·833
	23 July, 1801, 9 A. M.	60°	1259·666	1282·500
	——— 11 A. M.	63·3°	1262·000	1287·500
Mean of the three obs.		....	1260·166	1288·944
Huts of Meiri	22 July, 1801, 7 P. M.	60·8°	880·183	896·833
Nancy-sur-Closes	27 July, 1800, 8 A. M.	61·7°	461·166	467·833
Lake of Chéde	21 Aug. 1801, 2,15′ P. M.	78·1°	402·333	498·000
Huts of Villy	18 Aug. 1801, 8 P. M.	53·4°	947·266	964·666
Defile of Charlenton	19 Aug. 1801, 8 A. M.	53·7°	1270500	1296·166
Summit of Buet	——— 9,45′ A. M.	55·4°	1562·666	1596·000
	——— 11,20′ A. M.	54·9°	1585·166	1619·000
Mean of the two obs.		....	1573·916	1607·500
According to Mr. Pictet	July, 1778	66·8°	1580·000	1616·833
According to Mr. Deluc		....	1559·166	

According

PLACES.	Time of the observation.	Mean heat.	Accord. to Deluc.	Accord. to Trembley.
Valorcine	13 Aug. 1801, 9,15′ A. M.	59·3°	622·333	632·833
Town of Prieuré	20 Aug. 1801, 1,20′ P. M.	75·6°	543·000	552·500
According to Mr. Saussure		....	524·666	
and from a mean of 85 barom. obs.		73 9°	525·000	534·666
——— Mr. Saussure, jun.	2 Aug. 1787, noon	78·1°	527·666	537·166
	——— 2 P. M.	80·7°	534·666	544·500
Summit of mount Bréven	18 Aug. 1801, 12,30′	57·6°	1283·166	1309·323
Hut of Phinpra	——— 10 A. M.	59°	1052·000	1073·333
Huts of Arclevé	——— 4 P. M.	61·8°	956·000	975·333
——— Balme	12 Aug. 1801, 3 P. M.	70·9°	1019·000	1041·333
Department of Mount Blanc.				
Villaz	11 Aug. 1799, 7 A. M.	61·7°	381·166	385·833
Summit of Pormonaz	——— 3 P. M.	66·2°	919·500	938·666
	23 July, 1800, noon	59·8°	922·833	941·166
Mean of the two obs.		....	921·166	939·916
Huts on Villaz	11 Aug. 1799, 10 A. M.	65·5°	728·333	742·500
	22 July, 1800, 7 P. M.	59·3°	741·333	754·000
	23 July, 1800, 8,30′ A. M.	58·1°	741·833	755·333

Mean

PLACES.	Time of the observation.	Mean heat.	Accord. to Deluc.	Accord. to Trembley.
Mean of the three obs.		....	733·833	750·611
Lake of Annecy *	20 July, 1800, 6 A. M.	59·7°	229·000	229·666
Talloires, according to Mr. Pictet	16 Sept. 1790, 8,15′ P. M.	64·6°	238·500	239·233
Summit of la Tournette	13 Aug. 1799, 10 A. M.	60·7°	1183·333	1208·500
	21 July, 1800, 3 P. M.	64·5°	1199·666	1226·000
According to Mr. Pictet	17 Sept. 1790, 8,52′ A. M.	61·9°	1182·000	1207·333
	——— 10,40′ A. M.	66·4°	1190·166	1212·666
Mean of the four obs.		....	1190·211	1213·624
Hut of Cassay	21 July, 1800, 11 A. M.	60·6°	903·333	921·183
According to Mr. Pictet	17 Sept. 1790, 6,10′ A. M.	57·7°	882·000	899·000
Mean of the two obs.		....	892·666	910·641
Huts on Thônes	12 Aug. 1799, 7 P. M.	60°	809·666	825·166
	21 July, 1800, 7,45′ P. M.	58·1°	813·833	829·000
	22 July, 1800, 5 A. M.	48·9°	821·666	835·333
Mean of the three obs.		....	815·055	829·833
Huts of l'Eau, according to Mr. Pictet	17 Sept. 1790, 2,25′ P. M.	74·7°	712·833	727·333

* According to a mean between Saussure and Pictet, 222·666.

VIII.

VIII.

On the Cultivation of the Poppy. By T. COGAN, M. D*.

GENTLEMEN,

ALTHOUGH the ardour with which the British nation pursues whatever promises to be of public utility, is perhaps unequalled by any other, and certainly exceeded by none; yet there is one subject which has hitherto been permitted to escape our attention, and in which several nations upon the continent can not only boast of their superior policy, but are already enjoying considerable advantages from it; I mean *the cultivation of the poppy to a great extent for the benefit of its oil, as an article of food*, and for other useful purposes.

Cultivation of the poppy neglected.

It will doubtless be remarked, that we ought not to ascribe the neglect of it as an article of food to *inattention* altogether, but to a superior caution, as the narcotic quality of the poppy renders it totally unfit to be taken inwardly. This, it is allowed, is, in appearance, a very formidable objection; and as it respects the lives of multitudes, it ought not to be treated with levity: the objection itself, and the argument from analogy on which it is founded, ought to be completely confuted, before the article can be recommended to the community in this novel point of view.

Objection to it.

We might observe that the objection is solely founded upon very slight and imperfect analogy. It *assumes*, that, because some parts of a plant are noxious, the whole must be equally noxious. But this assumption may be confuted in numberless instances. Daily experience testifies, that different parts of plants possess not only different, but *opposite* qualities. Oranges and lemons, which are used in profusion, possess juices that are both palatable and refrigerating; but these are enclosed in a rind, the essential oil of which is extremely acrid and stimulating: and it is well known that the bland and nutritive tapioca is the produce of a tree the roots of which are highly poisonous. In this case, therefore, the ar-

Answer to this objection.

* Papers of the Bath and West of England Society, vol. X, p. 331.

gument

gument from analogy may be considered as a very proper motive for *caution;* but if it advances farther, it degenerates into a pernicious *prejudice.*

There have been, however, many incidental circumstances which have had a partial influence in removing these prejudices. It is well known, that compounders of medicine have made a very liberal use of the seeds of poppies, as substitutes for the oil of sweet almonds, without the least detriment to the patient. They have sometimes imputed to it *additional virtues*, from its being supposed to possess narcotic properties. But that they have erred in their hypothesis is plain, from the practice of many individuals, who have made the seeds of poppies a common article of food*.

The oil has been used freely without injury,

and the seeds also.

But it will be the principal object of the following paper to inform the inhabitants of this country, through the medium of your publication, that the above objection has been repeatedly advanced and repeatedly confuted; that experiments, first made with a degree of caution, have finally removed prejudices long and inveterate; and that the white poppy *(papaver hortense semine albo)* is cultivated to a very great extent in *France*, *Brabant*, and *Germany*, and more recently in *Holland*, chiefly to extract the oil from its seeds; which is found not only to be salubrious, but to be peculiarly delicate in its flavour. It is now become a considerable article of commerce: the oil of a superior quality, for the use of the table, and the inferior for manufactories and various other purposes. It is produced not only with considerable profit to the cultivator, but also to the merchant and consumer.

Cultivated extensively abroad for its oil.

As it is natural to imagine, that the prejudices against the common use of poppy oil for culinary purposes will be very general, since they are apparently sanctioned by prudent caution, it is not expected that the most positive assertions, founded upon the experience of strangers on the continent, would be sufficient to remove them. But a circumstantial narrative of a contest which has already taken place; and the final triumph of experience over the opposition founded

Prejudices against it,

Successfully combated.

* See Prosper Alpinus, lib. iv, cap. i. Geofrey Mat. Med. tom. ii, p. 715. Lewis's Materia Medica, Article Papaver Album.

on

on analogous reasoning; and a particular statement of the advantages which have accrued to the cultivator, merchant, and consumer, may perhaps attract the attention of some agriculturists in our own country, who may thus be encouraged to make similar experiments: and as the issue must be the same, they will be able to produce absolute demonstration, that the oil is totally destitute of the noxious qualities, that have been ascribed to it; and finally convince the public, that it may become a cheap and useful substitute for the olive oil, and a very beneficial article of commerce.

Rise and progress of its cultivation.

For this purpose I shall state to the agriculturist a succinct account of the rise and progress of the cultivation of the poppy, in order to express the oil from the seed; the manner of cultivating it, and the emoluments which have been received by the cultivator, from authentic documents in the Dutch and German languages which are in my possession.

Oil of poppies.

In the year 1798, the Society established at Amsterdam for the encouragement of agriculture, being informed that the oil of poppies was cultivated in several parts of *France*, *Flanders*, and *Brabant*, thought it an object of sufficient importance to make more particular inquiry; and they learned from indubitable authority, not only that it was generally used in the place of olive oil, but that several thousand casks of it were exported annually, a large quantity of which was imported into *Holland*, and sold under the name of olive oil, or mixed with it in considerable abundance; and they appealed to several merchants who were members of the Society for the truth of this assertion, without being contradicted.

Made in large quantities and sold as olive oil.

Premiums proposed by a Society at Amsterdam.

These facts induced the Society to propose three premiums, consisting of a *silver medal* and *ten ducats* each, which were divided into the three following classes.

The *first* to the husbandman who should sow not less than half an acre of a *clayey* soil with poppy seed; the *second* on a sandy ground; and the *third* on turf or peat land.

They also offered to the person who shall have cultivated the largest quantity of ground, on the two first species of soil, in the most masterly and advantageous manner, *a gold medal*, value *fifty ducats*, or that sum in money, in lieu of the above premiums.

The

The candidates were to give an accurate statement of the quantity of seed sown per acre; the time of sowing, and of gathering the poppies; the quality of the soil; the manner of procedure in every part of the process; the quantity of oil produced, and the total of the expenses.

Claimant for them.

In consequence of the above proposals, in the year following (1799) Mr. P. Haak became a claimant; sent in satisfactory specimens of the oil produced, accompanied with testimonies from two respectable physicians, that upon experiments made, it fully appeared that the use of the oil was not in the least prejudicial to the human constitution; and that the oil-cakes were very wholesome and nutritive food for cattle.

The Committee appointed to receive this report not only expressed their entire satisfaction at the attestations of the physicians, but they laid before the Society at large an account of the proceedings which had taken place in France, upon the interesting question concerning the noxious or salubrious qualities of the poppy oil, in the following *Narrative.*

Proceedings in France to ascertain the good or bad qualities of poppy oil.

So early as in the beginning of the seventeenth century, the oil of poppies was produced in such large quantities, that it gave rise to great and lasting contentions, which rose to such a height, that the government was desired to interfere, and appease the contending parties; either by authorising the use of this oil, or totally to prohibit the consumption, according as experiments should decide whether it contained the noxious qualities ascribed to it, or not.

Arguments against it.

The opposers urged the objections already stated: they asserted, that as the capsulum or poppy-head contained juices highly narcotic, this must also be the case with its seeds; that the frequent use of the oil extracted from them exposed the consumer to all the dangerous consequences arising from the too liberal use of opiates; and that they would finally obtund the faculties of the soul; that the oil was of a drying quality, for that it was upon this account it became peculiarly useful to painters: they therefore implored government to confine its uses to this object.

Answered by facts.

The *advocates* maintained that no proofs existed of these pernicious effects; but on the contrary, experience testified

that

that the seeds were peculiarly nutritive both to men and cattle; they asserted that the ancient Romans, concerning whose mental powers there could be no doubt, were accustomed to mix the oil and meal of the poppy seed with honey, and have it served up as a second course at their tables; and that it was on account of its nutritious qualities so well known to the Romans, that *Virgil* gives it the title of *vescum, food*, by way of preeminence; and that the peculiar qualities of this oil rendered it a desirable object of cultivation; and that taste was delicate and pleasant, somewhat resembling that of the hazel nut; that it continued in a fluid state, exposed to a much greater degree of cold than was required to congeal the olive oil; that it contained a larger quantity of *fixed air*, which preserved it a longer time from being rancid; that in these particulars it not only approached to the finest *oil of Provence*, but it mitigated the disagreeable taste which that oil acquired by length of time; and that the poppy oil decidedly deserved a preference to every other oil expressed from seeds, whether nut, almond, or beech; which, tho' they yielded large quantities, soon became rancid: and as there was no appearance of its being pernicious in the more extensive use of it, so valuable a product ought not to be confined within the narrow bounds of the painter's use.

Used by the ancient Romans.

Corrects the rancidity of olive oil.

Consequence of a scarcity of olive oil.

Things were in this state, without any prospect of accommodation between the parties, when the severe winter of 1709 overtook the combatants. This damaged the *olive*, *nut*, and *almond* trees to such a degree, that there was a great scarcity of their oils; and they were obliged to have recourse to the substitutes, beech and rape, &c. But it was soon perceived, that these were far inferior to the oil extracted from the red, white, or brown poppy, which had a much nearer resemblance to the small portion of the olive oil which the winter had spared. This was consequently mixed with the olive oil in the proportion of $\frac{1}{4}$, $\frac{1}{3}$, $\frac{1}{2}$, without the least opposition. But when it was attempted to sell the poppy oil in its pure and unmixed state, the opposition became so violent, that the Lieutenant-General of the police of Paris resolved, in the year 1717, to order the medical faculty of that city to make the

the strictest examination concerning this subject, and deliver in their report.

The faculty appointed *forty* of the most celebrated practitioners in medicine as a committee of inquiry, who were witnesses to various experiments accurately made, and whose report was expressed in the following terms: "*cum sensuissant doctores, nihil narcotici, aut sanitati inimici in se continere, ipsius usum tolerandum esse existimarunt*;" that is, they were of opinion, that as there is nothing narcotic or prejudicial to health contained in the oil, the use of it might be permitted.

Report of a committee of physicians in its favour.

But this decision was unsatisfactory; and popular clamours determined the court of justice to pass a decree in the year 1718, whereby the sale of poppy oil, whether mixed or unmixed, was prohibited, under a fine of three thousand livres for the first offence. Notwithstanding this prohibition, the sale of the article was clandestinely encouraged and gradually increased until the year 1735, when the court issued a severer decree, enjoining it upon superintendants appointed, to mix a certain quantity of the *extract of turpentine* to every cask containing 100lbs. of this oil; of which not less than two thousand casks were consumed in *Paris* alone. This attempt to render the use of it impracticable, had no other influence than to annihilate the public sale of the article, but the secret demand for it increased: till at length, in the year 1773, a society of agriculture undertook to examine with the closest attention all that had been alleged, either by writing or otherwise, for or against the general use of this oil. Experiments were repeated in the presence of the most distinguished chymists, with the same result, and the Society presented a petition to the Minister of Police, setting forth the great advantages that would accrue both to commerce and agriculture, by reversing the prohibition.

Popular clamours against it.

This petition was put into the hands of persons who vended various kinds of drugs, and who had, as a body, opposed the subject of it, with orders to state all their objections to the *medical faculty*; by these means the faculty became masters of every thing that was urged in the debate. They again made several experiments in the year 1776, and finally confirmed the decree of the faculty issued in 1717, declaring that

Final determination in its favour.

that the oil of poppies was not injurious to health, that it did not contain a narcotic power, and that it might be recommended to general use with the utmost safety. The medical faculty at *Lisle* had also made a similar declaration in the year 1773. From that time to the present the cultivation of the poppy has not met with any formidable opposition; and has increased to such a degree both in France and Brabant, that they have been able to export a considerable surplus, to the great advantage of the husbandman, as well as the merchant: and in seasons of scarcity it has been found of the most essential service, in all cases where the use of oils was required. In the northern parts of France, it was used by soap-boilers, as a substitute for other oils, which were extremely dear: and in *Brabant* the oil-cakes are constantly used as food for cattle with obvious benefit.

Oil used by soap-boilers.

Oil cakes for cattle.

These facts being established, the Committee of Agriculture in Amsterdam proposed the premiums above-mentioned, in order to ascertain whether the experiments made would authorize the cultivation of the article upon a large scale; whether the soil and climate of Holland were beneficial to its growth; whether the quantity or quality of the oil would be similar to the product of France and Brabant; whether the profits would indemnify the husbandman for giving it the preference to other crops; whether the oils could be afforded cheaper than those in common use; and to what purposes either in the arts or manufactories it might be applied.

Deeming it possible, that the narrative of a contest which subsisted the greater part of a century, and in which the advocates for the internal use of the poppy oil were uniformly triumphant, may have some influence in destroying our own prejudices and apprehensions, respecting the pernicious quality of this oil, I shall now proceed to state, in as concise a manner as perspicuity will permit, the most interesting particulars respecting its culture; selected from various foreign publications upon the subject.

Particulars of culture.

Soil.

Soil. The poppy may be cultivated with success on various kinds of soil. It has been tried on a rich black soil, peat-ground, and sandy heaths, and been productive. Those lands in which the wild poppy abounds the most, are obviously

ously most congenial to its nature. The richer the soil, and the clearer from weeds, the larger will be the crop. It is not so advisable, however, to manure for the poppy, as for the crop preceding it, as it is more exposed to injury by weeds. Hence it succeeds the best after carrots, cabbage, potatoes, &c. The land was generally prepared by the spade, as in planting potatoes; and the finer it is worked the greater the advantage. But when it is cultivated to a great extent, they use the plough. The seed has generally been sown broad-cast, the plants thinned, and weeded afterwards, as in the culture of turnips; but in drills it is sown about six or eight inches distant in the rows, which has been strongly recommended; experiments upon a small scale having manifested a superiority in this mode. Management.

The Kind and Quantity of Seed. Although the white poppy has been chiefly used in *France* and *Brabant*, under the supposition that it produced the finest oil, yet it has been found that various other kinds will answer the purpose as well. It is even asserted that the blue poppy, while it yields the largest quantity of seed, is in no respect inferior in the quality of the oil. Admiral *Kingsbergen*, whose private virtues render him no less a favourite with his countrymen, than his skill and courage as a naval officer, instituted an experiment with different kinds of seeds in the same soil, and he could not perceive any difference in the quality of the oil, while the seeds of the blue poppy yielded considerably more. What kind best.

The *quantity* of seed generally used in the broad-cast has been after the rate of 2 lbs. to an English acre. In drills a less proportion has been used. Quantity of seed.

Time of sowing. This is from the middle of March to the middle of April. If it be sown much earlier, it is more likely to be choked by weeds; if later, the harvest will be thrown deep into the autumn; and unless the weather be unusually favourable, the seeds will not ripen kindly. Seed time.

Weeding. As soon as the plants appear about two inches above the ground, they must be carefully weeded and thinned, till they stand about seven or eight inches from each other. The weeding to be repeated as often as it shall appear necessary. Weeding necessary.

Method of harvesting.

Harvest. In the beginning, middle, or end of August, according as the time of sowing has been earlier or later, and the season propitious, the seeds are ripe for gathering the poppy heads. Several methods have been recommended to harvest the crop. At first, the heads or balls were broken off from their stems, gathered together in large quantities, and deposited in a barn, or any other convenient place, in large heaps, in order to dry them. This method was not only tedious but injurious; some of the balls becoming musty, communicated a disagreeable taste to the seeds, and consequently to the oil. Mr. *Poske*, of Zell, in the electorate of Hanover, prefers the following method. He draws the entire plants out of the ground; binds a sufficient number of them at each extremity, and places them against each other in the manner of *wheat-sheaves;* and lets the whole remain in the field for eight or ten days, until they are perfectly dry. It was customary to cut open the capsulum with a knife; he prefers hacking it in two or three places with a *bill-hook*, and asserts that one person may in this manner do more work than ten times the number of hands in the former manner; and that the seeds are more easily evacuated from their cells. But the most convenient and expeditious method is to cut off the poppy heads, as they stand in the field: the reapers having an apron before them, tied up at the corners. In this they collect as large a number as is convenient, and empty them into bushel baskets placed upon a cloth; by which a considerable quantity of seed is saved. The heads are afterwards put into corn sacks, in a competent number to be trodden by men or children in *sabots*, or to be bruised by a mallet or flail: by these means the heads are confined from flying from the stroke, and the seeds preserved from being scattered, and afterwards passed through a sieve of a proper size.

Extraction of the oil.

In extracting the oil, it is of the utmost importance that the mill, press, and bags be perfectly clean and pure. *New* bags are necessary, as those used for linseed, rape, or any other seed, will communicate an unpleasant taste to the oil. It is adviseable to extract the oil as soon after the harvest as possible, as the seeds will yield a larger quantity than if deferred till the spring.

The

The first oil is destined for the use of families. This is *cold-drawn*, as any degree of warmth injures the flavour. After as much is extracted in this manner as possible, a considerable quantity of an inferior quality is obtained by heating the cakes, and pressing them a second time. Two kinds of it.

The oil expressed must remain for the space of five or six weeks before it is used, that it may deposit in a sediment a kind of milky substance that is mixed with it. It must then be poured into another vessel; and this should not be perfectly closed at first, but the opening be covered with a linen cloth, or a pricked bladder, that certain exhalations may pass. Nor should the oil be immediately used after the process is finished, as it continues to improve for a considerable length of time. Management of it.

That which is first expressed is of a pale colour; is peculiarly bland and soft, has a flavour approaching to that of the almond oil. It is used for sallads and other domestic purposes, either alone or mixed with olive oil. Should the latter be stale or rancid, it will be considerably improved by a mixture of recent poppy oil. It is not asserted that this oil may be placed in competition with *Provence* or *Italian* oils of prime quality; but that it is superior to the olive oils sold in shops, being often used to improve their quality. May I not add, that the inhabitants of this country are somewhat prepared for the culinary use of this oil, by being already accustomed to its taste, though without their knowledge. For since it has long been imported into Holland, and used without suspicion, we cannot suppose that the merchants of this commercial nation are totally strangers to the commodity*. The best oil. Superior to the common olive oil of the shops.

The

* We are told by Mr. C. A. Fisher, in his *Letters written during a Journey to Montpellier, in the year* 1804, "that the oil of *Provence*, which, on account of its purity, mildness, and fine flavour, is famous all over Europe, is exported to *Italy* in large quantities, and was formerly exported to many distant countries. But since the hard winters of 1789, and the following years, so many olive trees have been frozen, and during the Revolution so few planted, that *Aix* (which was the principal seat of its traffic) has now entirely lost its first and most lucrative branch of commerce.

 Two

Inferior useful for lamps, and other purposes.

The second-drawn oils are of a deeper colour, and are applicable to all the purposes of the more common oils. This may even be used as lamp oil; and it is alleged, that it does not give off so large a quantity of smoak, and emits a brighter flame.

Cakes equal to linseed for cattle.

The oil-cakes are peculiarly serviceable for feeding and fattening of cattle: being deemed equal to linseed cakes. All cattle are very fond of it, and eat it with eagerness.

Stems make fodder or manure.

This is the constant use of it in Brabant. The stems are sometimes used for fodder, containing a considerable quantity of nutritive oils; or mixed with stable dung and other manures, they enrich their quality.

Profit of this culture.

Expenses, *produce*, and *profits*. Concerning these articles it will be necessary to be particular, though it is somewhat difficult from a difference in the current coins, measures, &c. I shall state the result of experiments made on three hundred roeden†, about one acre, of a sandy soil, and three hundred roeden of a heavy peat, made by a claimant named S. N. Van Eys. The peat land being low and humid, he was obliged to make deep trenches between the beds. The harvest on this soil was later, the poppy heads were not so dry when gathered, and they shrunk considerably in drying. There was so small a difference in the quantity of seed from these different soils, that no important preference could be given. The sand ground yielded in this instance rather less than the peat land. As the quality of the seeds appeared perfectly similar, he mixed the whole produce together, when he sent them to the oil mills.

Produce of seed.

The produce of the sand ground rather exceeded 13 sacks, that of the *veen* or peat land, was about 12 sacks: together they made 25 sacks, 1 bushel of seed. These yielded oil in the following proportions:—

Two inferences may be drawn from the above information. Our best oils, though imported from *Italy*, are probably of the growth of *Provence*; and it is still more probable that the inferior sorts could not be afforded, even at the present price, without a large mixture of the *poppy oil*.

† The English statute acre is 160 square perches; and the Dutch morge, consisting of 600 roeden, is equal to 300 square perches: so that the difference between a Dutch morge and two acres, is as 300 to 320, the former being only 20 perches less than two acres.

23 sacks

Of oil and cakes.

	Mingles*.	Cakes.
23 sacks which were pressed *cold* gave ··	271	834
2 sacks *warmed* ······················	29	56
834 cakes *warmed* and pressed gave ······	73	
Total oil ··········	373	890
Cakes diminished in a second pressure to 726 ························· minus,		108
Total of cakes ······		782

Mr. Van Eys remarks that poppy oil of a very inferior quality is sold retail at one guilder, or 1*s.* 10*d.* per mingle or quart, and that mixed with olive oil at a much higher price. However he estimates the cold-drawn at 16*d.* only, and the second sort at 14*d.* per. mingle. The cakes are valued at 10 guilders, or 19*s.* per 100. His receipts stand thus:—

271 Mingles, (cold drawn) at 16*d.* ···············	F. 216	16	
102 ditto, (warm) at 14*d.* ····	71	8	
782 Cakes at 10 f. per 100 ····	78	4	
Total ··	F. 366	8	·· £33 0 8

Expenses.

STATEMENT OF EXPENSES.

To digging, &c. 600 roeden, at $1\frac{1}{2}$*d.* per r. ··········	F. 52	10	
Seed, sowing, weeding, &c····	42	19	
Harvesting, beating out seed, &c.····················	F. 48	3	
Pressing out the oil, bags, &c.	63	8	
Total ··	F. 207	0	·· £18 14 0

Receipts ···············	F. 366	8	0	·· £33	0	8
Expenses···············	207	0	0	·· 18	14	0
Total of Profit ··········	F. 159	8	0	£14	6	8

Observations.

This degree of profit upon nearly two acres does not at first appear to be encouraging: particularly if we take into con-

* A Mingle is about two pints.

consideration rent of land, taxes, &c., which are not mentioned in the statement. Mr. Van Eys has remarked, that the expenses attendant upon pressing out the oil, in this first essay, were considerably greater than would be experienced in the usual course of business. We may also notice, that the preparation of the ground by manual labour created a difference in the expense, that would prove an equivalent at least to the value of land and contingent charges. But what is of much greater moment is the very low price of the oil, as stated in the above account. That of an inferior quality being valued at somewhat less than 5*s* per gallon; and the superior at less than 5*s.* 6*d.*; whereas common lamp oil is with us sold for 6*s.* per gallon, and sallad oil of no extraordinary quality at 2*s.* 6*d.* or 3*s.* per pint, or 1*l.* or 4*s.* per gallon.

It clearly appears from these facts, that 1*s.* 6*d.* per pint, or 12*s.* per gallon for the prime article *wholesale*, and at least 4*s.* per gallon for the inferior sort, would be an advantageous price for the purchaser, who would be able to retail it considerably under the current prices of these articles.

Estimate to the English farmer.

According to this estimate, the receipts upon 271 mingelen or quarts of the *cold*-drawn would amount to about 40*l.*; upon 102 quarts of the inferior, to 5*l.*; and upon 782 cakes, at 1*l.* per 100, to 7*l.* 10*s.*; total 52*l.* 10*s.* for one *morge*, which would be after the ratio of 26*l.* 5*s.* per acre. The expenses not exceeding 10*l.* per acre, would yield a clear profit of 1*l.* 16*l.*

Should the oil of superior quality answer the description given of it, and be more palatable than the olive oil in common use, 12*s.* per gallon would perhaps be too low an estimate for our national character. For observation authorizes me to assert it as a serious fact, that nothing has a greater tendency with us to depreciate articles of nutrition, especially if they approach to luxuries, than to render them too cheap. And although we complain universally, that such articles are extravagantly dear, we almost as universally suspect or despise whatever may be purchased at a very reasonable price. But as retailers are both able and willing to obviate this objection, the above statement for the *vendor in wholesale* may be permitted to remain.

But

But there is another important point of view in which this subject may be considered. Successful attempts have lately been made to procure *opium* from the poppy, in no respect inferior to that imported from the East*; and it is asserted, that although it may be afforded at a very inferior price, the product would afford ample profit to the cultivator. As the opium issues from the *rind*, and the seeds have been proved not to partake of its narcotic properties, an important inquiry presents itself, *whether the poppy may not be cultivated with a view to both articles?* This can only be determined by solving another question, *will the incisions made in the green and unripe capsulum, and the exudation of its juices, prove injurious to the seeds in this advanced state of its growth?* The argument from analogy, which is the only mode until we can obtain facts, appears to favour the negative of the question; not only as there is no immediate correspondence in the qualities of these two parts of the same vegetable, but as many experiments have proved, that by checking the growth, or weakening the vegetative powers of one part of a plant, they are increased and improved in another.

Farther advantage in the opium to be obtained.

Desirous of obtaining some information concerning this interesting subject, I sowed, in the year 1804, about half a lug of garden ground with the white poppy seed; and when the heads were advanced to a sufficient state of maturity, I scarified the external surface of one portion of them with a penknife, suffering the others to remain entire; and though the exudations were very considerable, there was no perceptible difference in the colour, taste, or size of the seeds; excepting where the incisions passed through the whole integument, which frequently happened from the imperfection of the instrument, and my inexpertness. The seeds which lay nearest to the openings were discoloured by the admission of external air; but the taste of the seed was not injured.

Experiment.

This little experiment served to convince me, that the seeds of the poppy are peculiarly grateful to birds, rats, and mice. The first dexterously made large holes in the lower surface of

Rats, mice, and birds fond of the seeds.

* See Transactions of the Society instituted at London, for the Encouragement of Arts, &c. on the mode and advantages attending the cultivation of Opium.—Vols. 14, 15, 16, 18.

the

the ball, through which the seed fell to the ground; and they thus materially injured a considerable portion of my crop while it was standing; nor were the latter less destructive, when the poppy heads were spread upon the floor of the summer-house in order to dry them. I was however indemnified for this loss, by observing that not a single instance of mortality presented itself to evince the noxious quality of the seed.

If future experiments should prove, that both objects may be pursued by the same culture, scarcely any plan can be devised, which would prove equally profitable to the cultivator, and more beneficial to the community.

General reflections.

I am not so sanguine, gentlemen, as to expect that any person upon reading the above account will immediately resolve to cultivate the poppy to a great extent, as an article of profit. There is often a long repose between the acquisition of knowledge, and the application of it to practical purposes; and in this case I allow that many difficulties are to be surmounted, before the open and avowed consumption of this oil would be sufficiently extensive, to make the production of it an object of sufficient magnitude. But the increasing demand for oils of all sorts in our extensive manufactories, and by the daily improvements in our provincial towns, the immense sums expended in the importation of foreign oils, and most probably of this very oil under a false name, and the daily increase of their price, render a power in reserve most desirable. The time may arrive when the scarcity of oils for domestic use may increase to an alarming degree; in this case the general reluctance to the use of those which are now deemed of an inferior quality may in great measure subside, and we may perhaps rejoice at being supplied at a cheaper rate with that very oil, which passes smoothly among us under the ficticious character of *genuine oil of olives*. I shall at least enjoy the satisfaction of putting it in the power of the public to assist themselves at some future period; and take encouragement respecting the success of my endeavours from the nature of this very plant, which is frequently known to lie for years in the soil in a state perfectly inert, until some favourable circumstances may have promoted a vigorous

vigorous vegetation, to the surprise and alarm of the farmer, who has uniformly mistaken it for a *weed*.

N. B. It may be objected, that in the above estimate of the profits, mention is not made of the duties which may hereafter be imposed by government, and become considerable deductions. But this objection has no reference to our first essays. The duties will not become an object until the product of poppy oils shall sensibly diminish the importation of foreign oils; and in that case the wisdom of government will doubtless prevent their rising so high, as to operate as a discouragement to a culture, which would turn the balance of the oil trade in our favour; and should we be able to extend this culture so far as to export the article, a very moderate duty upon both home consumption and exportation may prove more than equivalent to the duties at present collected.

Since writing the above, I am informed by a person who deals largely in foreign oils, that letters from Leghorn announce an alarming deficiency in the last year's product; that the quantity is very small, and of a very inferior quality. This information should operate as an additional motive to the attempt recommended. The injury induced upon olive trees by inclement weather is frequently to such an extent, that it can only be repaired by the slow growth of new plantations. This circumstance gives an astonishing advantage to a substitute, of which, by its being an annual product, the deficiency of the most unfavourable year cannot be equally extensive, and would probably be supplied by the increased abundance of the year ensuing.

Advantage from being an annual.

IX.

IX.

On the Use of Tobacco Water, in preserving Fruit Crops, by destroying Insects; and on the Use of the Striped or Ribband Grass. By Mr. ROBERT HALLETT *.

SIR, *Axminster, April* 13, 1802.

BEING much engaged in mercantile concerns, and having but little time for other pursuits, I have not an opportunity, though extremely fond of my garden, of bestowing the attention to it I could wish; but having made a few experiments with a view of improving the state and bearing of my fruit trees, in which I have succeeded beyond my highest expectations, I hope, as my intention is to benefit the public, I shall be pardoned for troubling you with the present communication of them.

Popular notion of the decline of wall-fruit trees.

The old gardeners with us have long entertained an idea, that our climate has suffered a change particularly inimical to the successful cultivation of Wall Fruit Trees. To this circumstance they attribute the blight, which annually disappoints their hopes, and consider the evil beyond the reach of their skill to remedy.

Common disease of these trees.

The disease to which I have paid regard, is that which affects the trees in the early part of the season, curls up their leaves, often destroys the young shoot, and not unfrequently reduces the tree to a state of weakness, from which it is seldom to be recovered. I have, however, for some years past, successfully combated this baneful complaint, with a preparation easily to be obtained, attended with little expense, and yet certain in its effect.

Tobacco destructive to insects.

The efficacy of tobacco in destroying insects has no doubt been long known, and which I was well aware of. But as the expense attending its use, either for fumigating my trees, or washing them with an infusion, was considerable, and was perhaps the obstacle to its being generally resorted to, I endeavoured to find out the best method of obtaining it

* Papers of the Bath and West of England Society, Vol. X, p. 199.

it in quantities at a cheap and easy rate, of applying it with the least possible waste, and of preparing it so as to be used with safety. On considering the subject, it struck me that the tobacco water used by shepherds, having the power of curing the scab in sheep, might answer my purpose; and having a tobacco and snuff manufacturer very near us, I applied to him, and had the pleasure of finding, that in pressing his tobacco he obtained large quantities of it, which he threw away as useless, except some little which he sold to shepherds. This liquor was exceedingly strong; and, after various trials, I found, that a quarter of a pint, or indeed less if it was tolerably thick, would impregnate a gallon of water with sufficient power to destroy every insect or reptile that felt its influence; and that two gallons of it, when diluted, were enough to wash all my trees, which are about fifty, three times over, and to preserve them throughout the season in the finest health and vigour.

Water of tobacco & snuff manufacturers.

My method of using it has been thus: as soon in the spring as I observe a leaf on any of my trees begin to curl, or be in the least diseased, I prepare my tobacco water as I have before mentioned, viz. to something more than a wine-glass full, or nearly to a quarter of a pint of the liquor, I add a gallon of water; and mix it well together. I then sprinkle the whole of my trees over with this preparation, with a brush such as the plasterers use in moistening their walls, or sometimes by pouring it from a very small watering pot with fine holes; beginning at the top of the tree, and laying it on very gently to prevent waste, which would be considerable, if done with violence or thrown from an engine. Some time after, either in one, two, or three weeks, as I find it necessary, I repeat the sprinkling; and before the fruit gets to a size to be stained by it, I go over them again; and have always found three times sufficient to secure them from the depredations of the insect, which generally preys on the leaves before the shoots are much advanced in strength. I have now practised this antidote for some years; and having during that time taken every opportunity of communicating the knowledge of it, I have at this time the satisfaction of seeing it in such general use around me, that I find our tobacco manufacturer has such a demand for the liquor,

Method of using it.

liquor, that he sells it at 1s. 6d. a gallon. It may, however, no doubt, be obtained at a very cheap rate in Bristol, and other places, where much tobacco is manufactured. I would further add, that I have not confined my experiments of its use wholly to fruit trees. Every vegetable and shrub which I have applied it to have been relieved by it, and restored to health, though ever so much injured by the insect tribe. I have no doubt but it would effectually destroy the red spider; and that it may be used with salutary advantage in numerous other instances. And as it is a remedy so cheap, and attainable in any quantity that may be desired, I hope it will prove, on being generally known, beneficial to the public. How far it may be practicable to use it in hop-grounds, or in other extensive views, I cannot say. But I should imagine, as one watering only has a most powerful efficacy; and as the labour of one man would in a day go far in the application of it, that considerable benefit may be derived from it wherever the insect preys.

Destroys insects on all shrubs and vegetables.

Perhaps applicable in hop grounds.

I cannot dismiss my pen without mentioning a few words respecting another experiment, which my situation prevents my following up to the extent I could wish. I shall therefore briefly state, that observing from time to time the immense produce of an ornamental grass, which I had much of in my garden, and which is distinguished in Miller's Dictionary by the name of Striped or Ribband Grass; it occurred to me, that Nature in her bounty did not bestow such a prolific quality on this beautiful grass, but for some wiser purpose than merely to gratify the eye. I therefore examined it attentively, and found it to be very succulent, and possessing much sweetness; and on offering it to my horses and cows, that they fed on it very eagerly. I had by me a calf just weaned, which I kept wholly by it for a month; and notwithstanding it had so recently been taken from its mother, this grass supported it admirably, and I had the pleasure of seeing it thrive beyond my most sanguine expectations. I ascertained in the course of the year, that I could cut it three or four times, and that its produce was always prodigious. It takes a very deep root, produces an early spring crop, and, I believe, is an excellent summer food for cattle: in the winter it disappears. I should imagine

Ribband grass very prolific.

Excellent food for cattle.

Produce great.

gine it may be raised from seed; but I have found it to be easily propagated by dividing the roots into smaller plants, and disposing of them at distances of from four to six inches. In moist ground it spreads rapidly, and soon forms a thick mass of food exceeding any other kind I ever witnessed. Its durability is such, that what I have in my garden, which has been there to my knowledge these twenty years, is as thriving, and yields as much as ever.

Method of raising it.

Very durable.

I had need apologize for trespassing so long on your patience; and shall be happy if these remarks be in any degree found beneficial.

I am, with great respect, Sir,

Your most obedient servant,

ROBERT HALLETT.

[☞ It is presumed the destroying of insects, by sprinkling with tobacco water, is not new, though not generally practised; it may therefore be a public service to recommend the method. But as tobacco is comparatively a dear article, and the fluid above mentioned not easily procured in many situations, it might be a public advantage of no small importance, if our ingenious correspondents would turn their attention to the different tribes of vegetables, with a view to finding among our most pungent and bitter plants, or by a cheap and easy mixture of them, a substitute for foreign tobacco, for such uses. The idea is not without promise.

Perhaps some indigenous plant may be substituted for tobacco.

And further experiments on the striped grass are undoubtedly worthy of being made, in small enclosures near the farmer's or gentleman's house.—EDITOR.]

X.

A Second Letter from Mr. ROBERT HALLETT, *on the Efficacy of Tobacco Water in destroying Insects infesting Fruit Trees.*

SIR, *Axminster, Jan.* 14, 1804.

AS you requested in your favour of the 28th ult., that I would communicate to you any further discoveries that might have occurred to me in the use of the tobacco water for destroying

stroying insects on fruit trees, I trouble you with the result of my experiments last year, as it will strongly tend to confirm my representation to you on the subject, in the spring of 1802.

Experiments with tobacco water on fruit trees.

Being from home in the early part of last season, when my trees were putting forth their shoots, I was prevented the opportunity of applying the tobacco water as usual, and found on my return several large peach and nectarine trees, against a west wall in my garden, so wholly diseased, that not a leaf was to be found on them, but which was curled up and full of insects. The trees were then well covered with fruit of nearly the size of a hazel nut; but being destitute of leaves to shelter them, I despaired of saving any, and was apprehensive of losing even the trees. I immediately prepared some tobacco water, of more strength than usual, and applied it by sprinkling it very forcibly from a brush, and in two or three days I could perceive the insects were nearly all killed. I then renewed the application; and in about a week after I had the diseased leaves picked off, and repeated the wash, and found it to be thoroughly effectual. The trees completely recovered, put forth the finest shoots possible, and ripened an immense quantity of fruit in the highest perfection.

Ten years experience of its efficacy.

I have now used it about ten years, and am more than ever satisfied, that nothing can more effectually destroy every insect, that ravages on the leaves of trees and plants of all descriptions. And I conceive its benefits may be invaluable, if applied, as I observed in my former paper, to exterminate in hop plantations those insects, which are so destructive to the plants.

It being, as I have mentioned above, about ten years since I resorted to the tobacco water, and recommended it, its use has been gradually extending through this part of the country; and I observe, that some have mixed it with other things, and having found benefit from it, have considered their composition as an important discovery: but I am certain it has been several years longer in use by myself and my particular friends near me, than by any other person; and that it requires not any additional ingredient to render its good effects obvious, whenever properly applied. But I find,

Has been mixed with other things.

find, that as the demand increases, the tobacconists weaken what they send out; and care must therefore be taken, that it be sufficiently strong when used, which may be known by its giving the water a tolerably brown colour. I have found sometimes a wine-glass full sufficient for a gallon of water; at other times, what I have procured has been so much diluted by the tobacconist, that it has required a pint to give a proper strength to that quantity. I was last summer greatly annoyed by the red spider on those trees that had a direct south aspect. The minuteness of the insect, and being so securely sheltered underneath the leaf, prevented several of my applications from taking a due effect; but on watering my trees with an engine for about ten successive evenings, very forcibly, and immediately after being so watered giving them whilst wet a sprinkling of tobacco water, about three of those evenings, the trees recovered and ripened their fruit very finely.

It ought to be sufficiently strong.

Red spider.

I hope next summer to have it in my power to inform you of the result of an experiment I am now making, with respect to transplanting apple trees from an orchard near me, that is about to be converted to some other purpose. Having purchased as many of the trees as I was desirous of removing, I have newly planted out about forty of them, several of which bare each a hogshead of cider last year, and have done it in many previous seasons. As I have paid great attention to the preservation of them, I have little doubt of success. But as it cannot yet be ascertained, I shall defer enlarging upon the subject till I have the pleasure of addressing you again. In the mean time I remain with great respect,

Apple trees transplanted.

Your most obedient servant,

ROBERT HALLETT.

XI.

XI.

Remarks on a Pamphlet, lately published by the Reverend S. VINCE, *respecting the Cause of Gravitation. By a Correspondent.*

To Mr. NICHOLSON.

SIR,

Mr. Vince's pamphlet on the cause of gravitation.

IT is not long since I first saw Mr. Vince's pamphlet respecting Sir Isaac Newton's conjectures on the cause of gravitation; some parts of it appear to me so erroneous and so injudicious, that I think it right to take the first opportunity of expressing the disapprobation, which the author seems to deserve.

Cannot be the pressure of a medium varying in density as some power of the distance.

After having shown, that the established laws of gravitation cannot be derived from the pressure of a medium, of which the density varies simply as any given power of the distance, Mr. Vince proceeds in these words (P. 21), "It may be supposed, that if the above assumed law of density of the fluid will not answer the required conditions, yet some other law of density, which is compounded of different powers of the distance, may be made to agree with the law of gravity. Let us therefore represent the density of the medium by $Pa^m + Qa^q + Ra^s +$ &c.—Hence, according to the foregoing reasoning (taking only the two first terms of the series), the law of force tending to the sun is $P \times \frac{2m-mn}{3e} \times a^{\frac{2m-mn}{2}-1} + Q \times a \frac{2q-qn}{3e} \times a^{\frac{2q-qn}{2}-1} + R$, &c. Now these, being different powers of the distance a, *the whole can never constitute a power* which varies as $\frac{1}{a^2}$."

His mistake pointed out.

On this point the Professor's whole demonstration rests, and it is difficult to imagine how he could have committed so palpable a blunder. We have only to put $m = o$, and $R = o$, in order to show the fallacy of his reasoning: the force will then be respresented according to the expression here laid down, by $Q \times \frac{2q-qn}{3e} \times \frac{2q-qn}{2} - 1$, and $\frac{2q-qn}{2} - 1$ may become $= -2$ on many suppositions, while the

the density is expressed by two terms of the first series. But in fact there appears to be another mistake in Mr. Vince's calculations, for instead of $2m$ and $2q$, he ought to have written $3m$ and $3q$; Mr. Vince says, "let the density be as d^m, then — the distance of the particles is as $\frac{1}{d^{\frac{m}{3}}}$," (p. 17): Newton on the contrary, says, "particularum distantiæ erunt ut *cuborum* latera A B, *a b*, et mediorum densitates reciproce ut spatia continentia A B cub. et *ab* cub." II. 23. So that if the density be expressed by $P - \frac{Q}{a}$, n being -1, which is the power of the distance of the particles of an elastic medium expressing their repulsive force, the law of the derivative force will be represented by $\frac{2Q}{3ea^2}$.

Another errour.

Newton's assertion.

These errours in the work of a Professor of Astronomy and Experimental Philosophy, and a Professor in the University of Cambridge, afford no very flattering specimens of the mathematical attainments of this country: and I am sorry to say, that they have been passed unnoticed by one of the most respectable of our reviews, in which a copious account of the essay is inserted. "If the salt has lost its savour, wherewith shall it be salted?" Et quis custodiat ipsos Custodes?

Mistake unnoticed by the Review.

Mr. Vince has thought proper to complain, in his prefatory statement, of the conduct of the Council of the Royal Society, and in particular of that of its President, in declining to publish his essay in the Philosophical Transactions. He says, that it was presented by the Astronomer Royal to the Society, "when the President and one of the Secretaries requested, that the author would withdraw it, and present it again in the November following, as the paper appeared a proper subject for the Bakerian Lecture. It was accordingly withdrawn, and offered again at the time when it was requested to be presented. The paper was then read, and appointed to be the Bakerian Lecture. But *before* it went into the Committee which is expressly appointed to examine and determine what papers shall be printed, the author was informed, that it was doubtful whether his paper would be published. The circumstances attending this information

Complaint of the author against the Royal Society.

formation led him to suspect, that it would not appear in the Transactions of the Society, and in this he was not disappointed."

The Society vindicated.

In the whole of this important history there appears to me nothing whatever, that an impartial person could deem a just cause of offence. The author had more than once before been appointed to give a Bakerian Lecture; and when he offered this paper to the Society, "the President and one of the Secretaries" probably thought it a compliment due to his established reputation, to suggest to him, that it might serve for a Bakerian Lecture, without having gone farther than the title of his paper. He accordingly accepted the compliment and the fee. The paper having been partially read, as all mathematical papers must be, it is natural to suppose, that it was submitted to the examination of some one or more individuals, previously to its being discussed by the Committee of papers, since mathematical demonstrations cannot easily be examined by any large body of persons, however select; and as the opinion of such individuals might easily be expected to influence the determination of the Committee, it is not difficult to imagine, that it might be known beforehand, "that it was doubtful whether the paper would be published," although it may be questioned, whether or no the person who gave the hint acted with perfect discretion.

Farther remarks.

After these remarks on the mathematical parts of Mr. Vince's paper, and this attempt to explain the conduct of the Council of the Royal Society, it will scarcely be necessary to make any comment on the unjust and illiberal insinuation conveyed by the observation, that "Sir J. Pringle, the late *worthy* and *learned* President of the Royal Society, executed the duties of his high office with great *impartiality* and *honour*." Nor shall I enlarge at present on any other objections which might be made to Mr. Vince's essay: what he says respecting the interference of the ethereal atmospheres of the different planets is totally foreign to the question; and some others of his remarks, which are perhaps better founded, have already been stated by Professor Robison, and by other authors; but these are imperfections which

which might easily be forgiven, if they were the only errours that have been committed in the essay.

I am, SIR,

Your obedient humble servant,

3 *March*, 1808. DYTISCUS.

See *errata* at the foot of the last page.

XII.

Farther Experiments and Observations on Potash and its Base. In a Letter from Mr. C. SYLVESTER.

To Mr. NICHOLSON,

DEAR SIR, *Derby, March 28th*, 1808.

IN your Journal for February of this year, I communicated an account of some experiments, made, in company with my friend Mr. James Oakes, with a view to produce the metallic base of the alkalis, discovered by Professor Davy. In consequence of our not having sufficient galvanic power at that time, we did not succeed in separating the globules of metal from the potash, although we produced a substance, which detonated with a bright flash, when presented to water. We have however since repeated the experiments, with increased power, and have completely succeeded in producing the metal, detached from the alkali, in which it is imbedded. The result of these additional experiments I should, according to promise, have communicated for insertion in the succeeding number of your Journal; but, observing, both in your, and other periodical works, that the same result had been obtained by others, I conceived any farther detail unnecessary: as however we have paid attention to the production of the black matter alluded to in my last, which appearance has not been observed by any other experimentalist, I have thought proper to make a few additional remarks.

Detonating substance produced from potash.

Completely separated from it.

Black matter accompanying it.

After repeating the experiments several times, we ascertained the curious fact, that the black matter was formed at the

Formed at the copper end only.

the wire coming from the copper end of the apparatus only.

Did not appear to be carbon.

Suspecting from its blackness, that it might be carbon, we collected and dried a portion of it, which was subjected to the test of nitre in a platina spoon; we did not, however, observe the slightest indication of the presence of that inflamable body; but, since the quantity operated upon was very small, no *absolute* conclusion can be drawn from the experiment.

Cannot be an oxide of the base.

That it cannot be an oxide of the alcaline base containing less oxigen than constitutes alkalinity, appears from its remaining permanent in water, for several weeks after the experiment; a circumstance, which could not take place with any substance having so great an affinity for oxigen: It is equally evident, that it cannot be an oxide with more oxigen, because it is formed at the copper end of the battery. Is it any foreign matter, derived from the vegetable, whence the potash has been obtained?

Alkali formed by incineration of vegetables: but in what state does it exist in them?

It is a well ascertained fact, I believe, that vegetables furnish a greater quantity of alkali by incineration, than is to be found in their composition previous to the process. It would, therefore seem, that alkali is formed during the combustion, and that all of it does not exist in the vegetable in the state of alkali: nor does it exist in the state of its base, since this substance would be incompatible with the presence of the vegetable fluids. In what form, then, does it exist?

Alkalis compounds, and earths perhaps the same.

In consequence of the numerous confirmations of Mr. Davy's discovery, we may with some confidence conclude, that the alkalis ought no longer to be considered as simple bodies, and it is exceedingly probable, that the earths are also compounds of oxigen, united to certain inflammable bases; a circumstance long ago suspected by Lavoisier, and others.

Chemical nomenclature and system require a change.

The nomenclature and systematic arrangement of chemistry therefore must undergo an alteration, particularly that part of the former, which embraces oxigen and its compounds; since we find that substance to be as well the principle of alkalinity, as of acidity.

New arrangement.

Under the new arrangement all ponderable matter will most likely be divided into two classes of simple bodies, namely, oxigen and inflammable

flammable bodies; from which will result the following classes of compounds, first, all those formed by the union of inflammable bases, and secondly, the simple and compound oxides. The simple oxides will include all oxides, properly so called, the acids, the alkalis, and the earths; under the compound oxides will be comprised the various genera of neutral salts.

I am, Sir,
Your most obedient humble Servant,
CHARLES SYLVESTER.

A spontaneous explosion of the alkaline base, mentioned by your Correspondent, page 146 of the present volume, occurred to us; the effect of which fractured the glass tube in which the experiment was made.

Erratum. Vol. XIX, page 157, line 7, *for* "cock," *read*, "cork."

XIII.

An Account of the Measurement of an Arc on the Meridian on the Coast of Coromandel, and the Length of a Degree deduced therefrom in the Latitude 12° 32′. By Brigade Major William Lambton*.

IN a former paper which I had the honour to communicate to the Asiatic Society, I gave a short sketch of an intended plan for establishing a series of connecting points commencing from the Coromandel Coast, and extending across the Peninsula; but that paper was only meant to convey a general idea of the principles on which the work was to be conducted; a more circumstantial and scientific account, it was thought, would be more to the purpose, when I had the means of putting the plan in execution, and detailing the particulars. Since that time I have received a most complete apparatus, which has enabled me to proceed on the scale

Plan for measuring an arc in India.

* Abridged from the Asiatic Researches, vol. VIII.

scale I originally proposed, and what is here offered is the beginning of that work, being the measurement of an arc on the meridian, from which is deduced the length of a degree for the latitude 12° 32′, which is nearly the middle of the arc.

An account of the base line.

The place of the base.

Some time had been taken up in examining the country best suited for this measurement, and at length a tract was found near St. Thomas's Mount, extremely well adapted for the purpose, being an entire flat, without any impediment for near eight miles, commencing at the race ground, and extending southerly. This being determined on, and the necessary preparations made, it was begun on the 10th of April, and completed on the 22d of May, 1802.

Instruments employed.

I had expected a small transit instrument from England, for the purpose of fixing objects in the alignement, and for taking elevations and depressions at the same time; but that instrument not having arrived, I thought it unnecessary to wait, particularly as the ground was so free from ascents and descents; I therefore used the same apparatus as I had formerly done, viz. the transit circular instrument, and the levelling telescope fixed on a tripod with an elevating screw in the centre. In all horizontal directions, this telescope fully answers the purpose, and as there has been no deviation from the level to exceed 26′ 30″, excepting in one single chain, and those cases but very few, I feel entirely satisfied as to the accuracy of the whole measurement.

The chain.

The chain which was made use of is the one I formerly had; and I was fortunate enough to receive another from England, made also by the late Mr. Ramsden, and this having been measured off by the standard in London, when the temperature was 50° by Fahrenheit's thermometer, it afforded me an advantage of correcting for the effects of expansion, a circumstance in which I was by no means satisfied in the former measurement. In order, therefore, to have a standard at all times to refer to, I have reserved the new chain for this purpose, and used the old one only as a measuring chain, by which means I can always determine the correction for the wear.

There

There were only four angles of depression, and two of elevation, taken in the whole length of the base; the rest were all horizontal measurements, and many of them consist of a great number of feet before it became necessary either to sink or elevate the coffers; when that was done, great care was taken to mark the termination of the preceding measurement; and for that purpose a small tripod was used in the shape of a T, with three iron feet to run into the ground, the straight side of which T was placed in the line. Another small T was made with its top also parallel to the line, and fixed upon the large one so as to slide to the right or left, and upon that again was a long piece of brass made to slide out at right-angles to the top of the T; in the middle of this brass a mark was made, which was brought to a plumb line let fall from the arrow, and the height from the brass to the arrow was noted down; when the succeeding chain was laid, which was to commence the new level or hypothenuse, the arrow was then brought so, that a plumb line, freely suspended, would coincide with the mark on the brass slider. The height of that chain above the brass was likewise taken; by comparing these two heights the elevation or depression of the new commencement was determined; and these differences noted in the seventh and eighth columns of the table. The differences of the two aggregates contained in these columns, when applied to the ascents and descents, will therefore show how much one extremity of the base is above the other. The height of the chain at the commencement and termination of the whole was of course taken from the ground.

Proceeding on raising or lowering the coffers.

All the other particulars respecting this measurement are nearly the same as that in the Mysore country, a full account of which has been published in a former volume of the Asiatic Researches. Some little alterations have been made in the coffers; that is, they were all of the same length, and the whole together about ninety-six feet, so as to give room for the pickets with the brass register heads. Their sides continued to the ends, and their depth on each side was the same, for the purpose of being turned every day, that they might not fall into a curve by their own weight and that of the chain. I also used tripods with elevating screws

Coffers.

in

in the centre, for supporting the coffers, making no other use of pickets than for the drawing and weight posts, and for carrying the register heads. The top of each stand or tripod was a thick circular piece of wood, fixed firmly to the end of the elevating screw, and a slip of board was fastened across the circular top, screwed into the centre, and allowed to turn round. When the ends of two coffers were placed on the top piece, this slip of board was admitted into the under part of each, and prevented their sliding off, a precaution that was very necessary on account of the high winds.

Commencement of the base.

The point of commencement of the base was had by dropping a plummet from the arrow of the chain suspended by a silken thread. A long but small bamboo picket had been driven into the ground, till its top was level with the surface, and the cavity of the bamboo was such as just to receive the plummet, and when the first chain was in the coffers, drawn out by the weight at the opposite end, it was adjusted by the finger screw at the drawing post in such a manner, that the plummet might hang suspended over the cavity of the bamboo, while the thread was applied to the arrow. This was done within the observatory tent, that the plumb line might hang freely without being disturbed by the wind. The bamboo picket was preserved with great care during the time I was observing for the latitude, and was then protected under the frame of the zenith sector. When the tent was removed, a large bamboo flag-staff was erected, the cavity of which covered the picket, and in this state it remained until the measurement was completed.

Termination of the base.

At the termination of the base, being the end of a chain, one of the large hooped pickets was driven into the ground till its top was on a level with the coffers and under the arrow of the chain. The opposite end being adjusted by the finger screw, the arrow at the leading end was nearly the centre of the picket. A mark was made, and a small round headed nail was driven in till it was level with the surface. The chain was again applied, and the arrow cut the centre of the nail. The picket had been driven upwards of two feet and a half into very hard clay.

The extremi-

But that these extremities may be preserved, in case they may

may hereafter be referred to, I erected small masses of hewn stone eight feet square at the bottom and four at the top, the axis of these masses being made to pass through the points of commencement and termination, and in order that this might be correctly done, the following method was used. ties marked.

I marked out the foundation of the building, so that the picket might be as nearly in the centre of it as possible. The earth was dug about a foot deep, reserving a space round the centre untouched. After the foundation was brought to a level with the surface, the first tier of stones was laid, being one foot in height. The inner part was then filled up with stones and mortar, taking particular care at the same time, that the centre was not touched. The next tier of stones was then laid, which was six feet square and one foot high. This also was filled in with great care, and some cement and bricks put gradually round the picket. After that the last tier was laid, which was four feet square, and also one foot high. When these stones were firmly fixed, small silken threads were drawn across each other in the diagonals of the square. A plummet (pointed) was then suspended from the point of intersection of these threads, and they were so moved, that the point of the plummet coincided with the centre of the nail in the picket. The position of these threads being determined, marks were inserted in the stone. The cavity was then filled up, and a square thick stone was fixed in the middle of the mass, having a circular place of about four inches diameter, sunk half an inch deep, and the centre of which was marked by a point. This point, by moving the stone and again applying the silken threads, was brought to coincide with the point of intersection, and then it was firmly fixed and pointed.

Structure of the truncated pyramid at the end,

Precisely the same kind of building was erected at the beginning of the base, but, in place of having a picket in the centre, four large hooped ones were driven into the ground, forming a square of about ten feet, the small bamboo picket being intended as the centre. Silken threads were then drawn across from the diagonal pickets, and so moved, that the plummet first used, suspended from the point of intersection of the threads, might drop into the cavity

and of that at the beginning of the base.

vity of the bamboo. That being adjusted, lines were drawn on the tops of the pickets where the threads had been extended. The building was then erected, and the centre both of the second and last tier was marked by the intersection of those threads, when applied to the marks on the pickets.

Such has been the mode of defining the extremities of the line. The buildings are well built of stone and some brick, and will remain for years, if not injured by acts of violence. They are intended to receive an instrument on the top, and the points are points of reference, if it should ever be thought necessary to have recourse to them.

Expansion of the chains, and their comparative lengths.

Comparison of the chain used with a new standard.

As I wished to be satisfied with respect to the expansion of each of the chains, and their comparative lengths, I made a course of experiments for both purposes. I had accordingly the coffers arranged near the ground, that the drawing and weight posts might be driven deep and firmly fixed. Both the chains were then put into the coffers, and the comparisons made as follows:

April 10, at 6 P. M. the temperature by a mean of five thermometers was 85°·6.

Three comparisons were made, and the old chain exceeded the new one, nine divisions of the micrometer screw.

April 10, at 6 A. M. the temperature by a mean of five thermometers was 79°.

Four comparisons were made, and the old chain exceeded the new one nine divisions. Therefore at the commencement, the old chain exceeded the new one in length, nine divisions of the micrometer.

May 23. After the base was completed, the temperature by a mean of five thermometers, was 86°.

By a mean of five comparisons, the old chain exceeded the new one .... 10·65 divisions.

24. The temperature by a mean of five thermometers was 84°.

And

And a mean of six comparisons gave the excess of the old chain above the new one···· 11·08 ditto.

25. The temperature was 87°.

And a mean of two comparisons, gave················ 11·00 ditto.

Mean ···· 10·86 ditto.

Hence it appears, that, at the conclusion of the base, the old chain was longer than the new one 11 divisions of the micrometer very nearly, so that it had increased, from being in use, 2 divisions, or $\frac{2}{700}$ of an inch.

These experiments were made with great attention, and when either chain was stretched out by the weight, it was carefully brought into a line in the coffers.

As I had reserved the new chain for a standard, and knowing the temperature at which it had been measured off in London, I considered it an object to determine its rate of expansion and contraction compared with the thermometers which had been in use in measuring the base, since these were but common ones, and might probably differ from those made use of by General Roy and others, who had determined the expansion of metals by the pyrometer; and I was farther induced to do this, from seeing the great variation among them, when the degree of heat became above one hundred, which it generally was in the coffers every day before I left off. To avoid those irregularities arising from the expansions being checked by the resistance from the pressure on the coffers, I chose the times of sunrise, and from one to two o'clock, P. M. for making the observations. Sunrise in India is generally the coolest time of the twenty-four hours, and the chain had during the night, on account of the uniform state of temperature, full time to free itself from any resistance. At the hottest part of the day likewise there is a considerable time when the thermometers are nearly stationary, which will afford time for the resistance in the coffers to be overcome; and it is necessary to pay particular attention to this circumstance, for the chain will be perceived to lengthen often for nearly half an hour after the thermometers are at their highest.

Rate of expansion.

Time allowed to obviate effects of friction.

I had

I had made a great many experiments prior to the measurement, but found great irregularity, partly from not attending sufficiently to the above circumstance, and partly from the unsteadiness of the drawing post, notwithstanding it was driven deep into very hard ground, and secured, as I thought, by having large stones pressed close on each side of it. To remedy this latter inconvenience, I had a staple driven into a brick wall, into which the iron was fixed with the adjusting screw for the chain, after which I perceived a perfect coincidence with the arrow and mark on the brass head, except what arose from the trifling expansion and contraction of the iron which held the chain. I then began a new course of experiments on both the chains, and the results were as follows.

Expansion of the standard chain.

Experiments for determining the expansion of the new Chain.

1802. Month.	TIME.	Mean of 5 Thermometers.	Change of Temperature	No. divisions.	Total expansion and contraction.	Total due to 1°.	Remarks.
					Inches.	*Inches*	
June 4.	2 P. M.	116·4					
5.	⊙ rise.	83	33·4	51	·245157	·00734	Weather
	2 P. M.	123·8	40·8	64	·307648	·00754	clear and
6.	⊙ rise.	82·5	41·3	64	·307648	·00744	windy
14.	⊙ rise.	80					during
	2 P. M.	119·1	39·1	60	·288420	·00737	the whole
15.	⊙ rise.	81·4	37·7	57	·273999	·00727	of these
	2 P. M.	121·9	40·5	63	·302841	·00747	experi-
16.	⊙ rise.	79·7	42·2	66	·317262	·00752	ments.
					Mean	·00752	

Experiments

Experiments for determining the expansion of the old Chain.

Expansion of the measuring chain.

1802. Month.	TIME.	Mean of 5 Thermometers	Change of Temperature.	No. divisions	Total expansion and contraction.	Total due to 1°.	Remarks.
June 8.	⊙ rise.	83·5	26·8	42	·201894	·00749	Cloudy
	2 P. M.	110·3	25·1	40	·192280	·00766	weather
9.	⊙ rise.	85·2	24·8	39	·187473	·00755	and high
	1 P. M.	110					winds du-
12.	⊙ rise.	80·2	27·9	42	·201894	·00724	ring the
	2 P. M.	108·1	24·8	38	·182666	·00736	whole of
13.	⊙ rise.	83·3	28	42	·201894	·00721	these ex-
	2 P. M.	111·3	31·3	46	·221122	·00706	periments.
14.	⊙ rise.	80					
					Mean	·00737	

It appears from these results, that the expansion due to 1° of the thermometer is less than what has been allowed by experiments made in England; but this might arise from the thermometers, as they were such as could be purchased in the shops, and therefore most probably not of the best kind. Great care, however, was taken to watch the moment when they stood the highest, and though they varied from one another considerably at that time, yet that variation was generally the same in equal temperatures.

Expansion less than allowed in England.

(To be concluded in our next.)

SCIENTIFIC NEWS.

Wernerian Natural History Society.

A Society has been established at Edinburgh for the cultivation of the different branches of natural history. It has been denominated the *Wernerian Natural History Society*, in honour of Werner. The following gentlemen have been elected office bearers.

Wernerian Natural History Society at Edinburgh.

PRESIDENT,

PRESIDENT,

Robert Jameson, *Esq. F. R. S. Prof. Nat. Hist. Edin.*

VICE PRESIDENTS,

Wm. Wright, *M. D. F. R. S.*	John Barclay, *M. D. F. R. S.*
Rev. T. Macnight, *F. R. S.*	Tho. Thompson, *M. D. F. R. S.*

Patrick Walker, *Esq. Treasurer.*

Pat. Neil. *Esq. Secretary.*

Council,—nine in number, viz. The above office bearers, with Charles Anderson, *Esq. F. R. C. S.*; and Lieut. Col. Fullerton, of Bartonholm. Sir Joseph Banks, *President of the Royal Society of London*; Richard Kirwan, *Esq. President of the Royal Irish Academy*; and Professor Werner of Freyberg, were elected honorary members. The following foreign members have been elected. Professor Karsten, Berlin; Professor Klaproth, Berlin; Mr. Von Humboldt, Berlin; Mr. Von Buch, Berlin; Mr. F. Mohs, of Stiria; Mr. Herder, Mr. Friesleben, and Mr. Meuder, of Saxony.

Two orders of veins of minerals.

At the last meeting of the Wernerian Natural History Society, Professor Jameson read a description of contemporaneous or enclosed veins. He divided veins into two classes. The first class comprehends *true veins*, the second *contemporaneous or enclosed veins.*

True veins characterized.

True veins, he remarked, excepting when the strata or beds are of uncommon thickness, traverse many different strata or beds; and, although we do not always observe them open at the surface of the earth, they invariably open at the surface of the formation or series of formations they traverse; thus the outgoings or openings of certain metalliferous veins, that traverse clay, slate, and mica slate, are sometimes covered by the second porphyry formation.

Contemporaneous or enclosed veins.

Contemporaneous or enclosed veins are in general confined to individual beds or strata, and are completely enclosed in them, or in other words wedge out in every direction in the bed or stratum in which they are contained. After detailing the various characters of true and contemporaneous veins, the Professor next described the contemporaneous veins

veins that occur in different great rock-formations, beginning with granite, and ending with the newest flœtz trap formation. He next explained the mode of formation of these veins. When describing the contemporaneous veins, that occur in gneiss, he remarked, that certain varieties of venigenous gneiss bear a striking resemblance to granite, and hence have been frequently confounded with it. This led him to point out the characters by which true granite veins are distinguished from veins of granitic gneiss.

Remark on the Huttonian theory.

As connected with this part of the subject he examined the facts, on which the Huttonian theory of granite is founded; and proved by a detail of his examination of the appearances described by Dr. Hutton, Professor Playfair, and others, that the supposed granite veins, shooting from subjacent granite into superincumbent rocks, are merely veins of granitic gneiss accidentally in contact with granite.

Professor Jameson's Elements of Geognosy, or 3d vol. of his System of Mineralogy.

Professor Jameson has just published the third volume of his System of Mineralogy, under the title *Elements of Geognosy*. The contents of this valuable work are as follows. Chap. I, Description of the surface of the earth; chap. 2, Effects of water on the surface of the earth; chap. 3, Internal structure of the earth; chap. 4, General account of the different formations in regard to their succession and stratification, and this illustrated by a short description of the Hartz and Saxon Erzebirge; chap. 5. Theory of the diminution of the waters of the globe—Description of overlaying formations—An investigation of the original contents of the waters of the globe, during the different periods of the earth's formation. The division of rocks into five classes; chap. 6, class 1, Primitive rocks; chap. 7, class 2, Transition rocks, chap. 8, class 3, Flœtz rocks; chap. 9, class 4, Alluvial rocks; chap. 10, class 5, Volcanic rocks; chap. 11, Mineral repositories; chap. 12, Relative age of metals, and general inferences. These are followed by a table of 32 pages, containing the relative antiquity and geognostic relations of simple minerals: also an extensive table of the most

most remarkable heights of mountains, hills, and lakes in different parts of the world, and a table of volcanoes. The volume is concluded with a series of notes explanatory of passages in the text, and referring to the Huttonian theory of the earth.

TO CORRESPONDENTS.

It would be highly gratifying to the author of this Journal, to publish a complete Index of the whole to the present time; and there is no motive for hesitation, but the probability, that the heavy expense attending it might not be indemnified in the actual sale. It is, however, under consideration.

The Meteorological Journal will appear in the first number of the next volume; and every attention that circumstances can admit will be paid to the suggestions received in the favour from an anonymous correspondent.

The errour of a word which he notices, is of the press, and we trust that errours of this description are not very frequent with us.

The letter from Mr. Garnett, of New York, was received too late for insertion this month, but will appear in our next number. His favours will be always acceptable. The enclosure to the Astronomer Royal was immediately forwarded.

ERRATA.

Page 304, l. 10 from bottom, read

$$P \times \frac{2m - mn}{3e} \times a^{\frac{2m - mn}{2} - 1} + Q \times \frac{2q - qn}{3e} \times a^{\frac{2q - qn}{2} - 1} + R, \&c.$$

l. 7 from bot. for $\frac{1}{a2}$ read $a^{\frac{1}{2}}$

line 2 from bot. read $Q \times \frac{2q - qn}{3e} \times a^{\frac{2q - qn}{2} - 1}$

A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

SUPPLEMENT TO VOL. XIX.

ARTICLE I.

Remarks on the total Eclipse of the Sun, June 16, 1806; with some new Methods of finding the Sun or Moon's Meridian Altitude, and the approximate Time, by Altitudes taken near the Time of Noon. In a Letter from J. GARNETT, *Esq. Editor of the American Nautical Almanac.*

To Mr. NICHOLSON.

SIR,

I AM a constant reader of your valuable Journal, but have only lately received your No. 75, in which, from the proceedings of the French Institute, you have copied Mr. Ferrer's observation of the total eclipse of the sun at Kinderhook. As I assisted him in the observation, I beg leave to remark a considerable errour, made by you or the French Institute, which places Kinderhook upward of 7^h to the eastward of Paris, instead of upward of 5^h to the westward.

Mistake of the reporter of the proceedings of the French Institute.

	h.	m.	s.
You mark the time of the conjunction	11	45	33 *
whereas it was, apparent time -	23	25	33·2,

as you will perceive by the printed calculation enclosed.

Before

* I copied this time from the *Magazin Encyclopédique,* and on referring to the *Journal de Physique,* where there is likewise a brief

Limb of the moon illuminated before the end of the eclipse.

Before the end of the total eclipse, the west limb of the moon began to be illuminated, and the light increased so rapidly, that I at last mistook it for the sun's egress, and called the time to Mr. Ferrer: but he saw the errour, and still kept his eye to the glass, when the first solar ray nearly blinded him.

Whence this?

Whence could proceed this illumination? from a lunar or solar atmosphere?

American Nautical Almanac.

In the American Nautical Almanac, which I have published here since 1803, I have given the moon's declination for every six hours, instead of twelve; which I did before I knew it was done in France, and for the same reason.

I am, with the greatest esteem,

Sir,

Your obedient Servant

JOHN GARNETT.

New York, North America.
February, 6, 1808.

brief notice of it, I find the time set down $11^h\ 25'\ 33''$. This is evidently according to the popular, not astronomical notation of time; and in a work intended for the general reader, as well as the astronomer, it was perhaps preferable. It appears however to have occasioned the errour of the French reporter of the proceedings of the National Institute.

ELEMENTS

ELEMENTS of the TOTAL SOLAR ECLIPSE, of June 16th, 1806, as observed at Kinderhook south landing, on the Hudson River, in latitude 42° 23′ 3″ N.; and longitude 73° 48′ 37·5″ W. from Greenwich.

Elements of the total solar eclipse, of June 16, 1806.

At the several contacts.	First.			Second.			Third.			Fourth.		
	h.	m.	s.	h.	m.	s.	h.	m.	s.	h.	m.	s.
Mean times of observations -	21	49	37	23	8	2,	23	12	39	0	33	45
Apparent times - -	21	49	31,1	23	7	55,4	23	12	32,4	0	33	37,7
Longitude by estim. *West* -	4	55	15									
Time at Greenwich -	2	44	46,1	4	3	10,4	4	7	47,4	5	28	52,7
Sun's R. Ascen. N. Alman. -	5	36	48,6	5	37	2,1	5	37	3,1	5	37	17,1
R. Ascen. Med. Cœl. $a+b$ (−24h)	3	26	19,7	4	44	57,5	4	49	35,5	6	10	54,8
The same in degrees - -	51°	34′	55″	71°	14′	22″	72°	23′	52″	92°	43′	42″
Altitude of Nonagesimal -	67	20	24	70	18	4	70	24	51	71	13	55
Longitude of Nonagesimal -	60	5	1,5	75	20	39	76	14	50	92	8	2
Moon's tr. long. * N. Al.—1′ 5″, 3	83	45	50,2	84	33	46,5	84	36	36	85	26	13,1
Sun's tr. long. * N. Al.—5″, 3	84	40	46,0	84	43	53,1	84	44	4,1	84	47	17,7
Moon's tr. long.—Sun's true long.	—	54	55,8	—	10	6,6	—	7	28,1	+	38	55,4
Moon's tr. dist. from Nonagesimal	23	40	48,7	9	13	7,5	8	21	46	—6	41	48,9
Moon's Hor. Par.—Sun's Hor. Par.	0	60	0,8	0	60	2,3	0	60	2,4	0	60	2,9
Moon's Parallax in Longitude	+	22	34,5	+	9	12,29	+	8	21,79	—	6	44,59
Moon's app. dist. from Nonages.	24	3	23,2	9	22	19,79	8	30	7,79	6	35	4,31
Moon's ap. long —Sun's ap. long.	—	32	21,3	—	0	54,31	+	0	53,69	+	32	10,81
Moon's tr. lat. N. Alm. North	0	24	56,35	0	20	30,66	0	20	14,98	0	15	39,67
Moon's Parallax in Latitude	—	23	5,49	—	20	13,99	—	20	7,44	—	19	23,17
Moon's Appar. diff. Latit. N. A.	Nor.	1	50,86	N.	0	16,67	N.	0	7,54	Sou.	3	43,5
Moon's aug. sem.—2″,977 infl.	0	16	38,373	0	16	40,253	0	16	40,283	0	16	40,803
Sun's sem. N. A.—1,623 irrad.	0	15	44,457									
Moon's relat. vel. in 12h. from Sun	6	51	23,8	6	51	45,3	6	51	46,7	6	52	8,4
Moon's do. between obs. and conj.	6	51	37	6	51	47,8	6	51	48,4	6	51	59,3

* These are the supposed errours in the tables of the Sun's and Moon's longitude, deduced from the observations with the estimate longitude of Kinderhook; but a corresponding observation at Greenwich will be more accurate: allowing for these, the time of the true conjunction at Greenwich by the Nautical Almanac will be 4h. 20m. 50·5s., when the relative velocity in twelve hours was 6° 51′ 50·14″.

New Methods of finding the Sun or Moon's Meridian Altitude and the Approximate Time, by Altitudes taken near the Time of Noon, recommended to the Practice of Seamen:—particularly the first.

PROBLEM I.

Method of finding the meridian altitude, and approximate time from two altitudes.

Given two altitudes of the Sun, within half an hour of Noon, and the Interval of time between; (the latitude by account and declination also given) to find the time and Merid. Altitude, by the help of table XVIII in the American Requisite Tables.

Example—In latitude 51° 30′ N. by account, and sun's declination 3° 30′ S. the sun's altitude was observed 35° 8′ 40″; and 17m. minutes after the altitude was 35° 7′ 20″; required the time from noon and meridian altitude.

		°	′	″	
Greater altitude	A	35	8	40	
Less altitude	B	35	7	20	
A − B	C	0	1	20	= 80″
			m.	s.	
80 ÷ 51. or C ÷ P	D		1	34	quotient in minutes.
½ interval	E		8	30	
1‡ E∾D	F		6	56	time (1‡) *before* noon, altitude A.
E + D	G		10	4	time (1‡) *after* noon, altitude B.
(tab. XIX) 2‡ $F^2 \times N$	H		1′	12″	the correction of A.
H + A	I	35	9	52	MERIDIAN ALTITUDE.

		″
Tab. XVIII. lat. 51° 30′ N. dec. 3 30 S.	N	1,5
Double interval of time	O	34m.
N × O	P	51′

1‡ When E is greater than D, the altitudes are on different sides of noon.

2‡ GG × N would also give the correction which added to B gives the Meridian Altitude, and when F or G is less than eleven minutes, table XIX gives FF or GG.

N. B.

N. B. The number of seconds in table XVIII may be found independent of the table, thus; add the constant log. 0.29303, the log. cosine of latitude by account, and log. cosine of declination together; and subtract the log. sine of the *difference* or *sum* of the latit. and declin. according as they are of the *same* or *different* names; the remainder will be the logarithm of the number of seconds in table XVIII.

Method of finding the meridian altitude and approximate time from two altitudes.

If to this log. be added twice the log. of any number of minutes less than 30 minutes, the sum will be the logarithm of a number of seconds; which added to the altitude, taken at that number of minutes from noon, will give the *meridian altitude*, the same as above.

REMARKS.

1. If the number of seconds that the sun or moon's declination changes in one minute, be divided by twice the number of seconds found from table XVIII, the quotient in minutes will be the *Correction of Noon*, from equal altitudes, for any less interval of time than twenty minutes.

2. This *correction* is also the time, in minutes, between the sun and moon's *greatest altitude* and *meridian altitude*.

3. And if *this correction* be multiplied into half the number of seconds that the declination varies in one minute, *the product* will be *the difference*, in *seconds*, between the sun or moon's *greatest* and *meridian altitude*, which, respecting the moon, is sometimes considerable; therefore as the altitude found by this problem H+A is strictly the *greatest*, not the *meridian altitude*, and the time F and G is the time before and after the *greatest altitude*: this product should be applied as a correction to reduce the moon's altitude when thus found, to her true meridian altitude, when that is required.

See also Remark 2 on the page next but one.

PROBLEM

PROBLEM II.

From three altitudes taken at equal intervals;

Given Three Altitudes of the Sun's Limb, taken near Noon, at equal Intervals of Time, to find the Meridian Altitude.

Example.—Suppose that three altitudes of the sun's lower limb were taken at 15 minutes interval *near noon*: as follows.

Time by watch.		Altitudes.		Differences.	Sum or differ.
h. m.		° ′ ″		′ ″	′ ″
11 56	A	59 55 0	B∽A a	3 40	$b \pm a$=E 22 20
12 11	B	59 58 40	C∽B b	18 40	(*When* B *is the greatest altit.* +,
12 26	C	59 40 0	C∽A D	15 0	*otherwise* - - ∽)

Then 2E : ½D :: ½D : correction of altitude B for meridian altitude.

And 2E : ½D :: double interval : time from noon of alt. B (*before* noon when A is *less* than C) which may be worked by the pen, Gunter's scale, or thus, by *prop. logarithms.*

2 E	44 40	P. L.	e	.6053	(*When* A *is less than* C, B *is* before *noon.*)
½ D	7′ 30″	P. L.	d	1.3802	
$d-e$	-	-	f	.7749	
$f+d$=G	1 16	P. L.		2.1551	B+G=Meridian Altitude 59° 59′ 56″ (*of sun's lower limb.*)
double int.	30m.	P. L	h	.7781	
$f+h$=I	5m. 2s.	*P. L.*		1.5330	I = Time *after* noon of alt. B, 5m. 2s.

PROBLEM

and from three altitudes taken at unequal intervals.

PROBLEM III.

Given Three Altitudes of the Sun's Limb, taken near Noon, at unequal intervals of Time; to find the Meridian Altitude.

Example.—Supposing the following altitudes and the intervals 29m. and 42m.

Interv. m.		Altitudes.		Diff. Int.								
p 29	A	54°	7	$128'\frac{1}{2}\div 29$	a	4′.431	$\frac{c}{p+q}$	D	0′.032	log.	d	8.5060
	B	56	$15\frac{1}{2}$	$90'\frac{1}{2}\div 42$	b	2.155						
q 42	C	57	46	1§ $b\pm a$	c	2.276	$D\times p$	E	0.928			
1§ *If* B *is greatest* +, *otherwise* ∽.						2§	$a\pm$ E	F	5.359			
2§ *When* B *or* C *is greatest* +, *or otherwise* ∽.							$\frac{1}{2}$F	G	2.680	log.	g	0.4281
H=minutes *bef.* noon of alt. A=1h. 23,6m.							$g-d$	H	83, 6m.	log.	h	1.9221
A+I=Meridian Altitude 57° 51′						-	$h+g$	I	224′	log.	i	2.3502

REMARKS.

‡1. When E exceeds a; A and B are on *different* sides of noon, and it is always to be preferred to have an altitude on each side of noon.

2. The nearer the sun passes the zenith, the nearer should the observations be taken to noon: generally the greatest horary angle should not exceed half the meridional zenith distance; but the differences between the observed altitudes should be sensibly greater than the errours which may be committed in the observations, and the intervals of time may be from 10 to 30 minutes.

3. Without logars. G÷D=H; and H×G=I. If the altitudes are taken to exact minutes of time, as in all these examples, it will make the operation more easy.

4. To prove the operation, if D×q be put for E, and $b\pm$E for F (which is the same as making C the first term) the merid. alt. will be C+I, the same as before, if rightly done; In the second example I would be found 5′ the correction of altitude C.

II.

An Inquiry into the Causes of the Decay of Wood, and the Means of preventing it. *By* C. H. Parry, *M. D.**

Highly important to preserve wood from decay.

THE power of wood in different forms to supply luxury, to promote science, and to guard and prolong human life, has made the means of preserving it from decay highly interesting to mankind. With this view various premiums have been offered by this and other œconomical societies. The object of the following discussion is to suggest the best means of prevention, chiefly by inquiring into the nature and sources of the evil against which it is intended to guard.

Two causes that destroy it.

Wood, when killed by being separated from its root, is subject to gradual destruction from two causes,—rotting, and the depredations of insects.

Two kinds of rot;

Of the rot there are two supposed kinds, as they affect wood, first, in the open air, or secondly, under cover.

wet,

The first is that which in the terms of our premium, Class VII, No. 3, is said to occur to "barn and other outside doors, weather-boarding, gates, stiles, and implements of husbandry:" To which, if there were any need of this minute specification, might have been added posts, rails, paling, water-shoots, and various other objects.

and dry.

The second is well known under the name of the dry-rot, the cause and prevention of which are the subjects of a premium by the Society of Arts in London.

Dead matter subject to decomposition,

Animal and vegetable substances possess certain common properties and movements, which constitute what is called life. When that state ceases, and these properties and motions no longer exist, the bodies become subject to the chemical and mechanical laws of all other matter.

only under certain circumstances. Fish and other animal matter

When perfectly dry, and in certain degrees of temperature, both seem to be scarcely capable of spontaneous decay. On this principle vast quantities of salmon are annually conveyed in a frozen state to London from the north

* From Papers of the Bath and West of England Society, vol. XI, p. 226.

of

of England and Scotland; and the inhabitants of the still more northern regions constantly preserve their food, by freezing, unchanged through the longest winters. The gelatinous and other soluble parts of animal substances, when extracted by boiling, and kept in a soft moist state, very readily putrefy. But if the same matter be dried by a gentle heat, and secluded from moisture and air by being kept in bottles or metallic cases, it will remain very long without decay. This is the theory of that well-known and useful substance, portable soup. In the burning climate of Africa, when it is intended to preserve a dead animal for food, all that is necessary is to cut the muscular parts into thin strips, from which, in a few hours, the heat of the sun exhales all moisture, reducing them to a substance like leather or horn, which proves to be unsusceptible of future decay from putrefaction. So also entire human bodies, buried in the arid sands of those countries, have often been found converted by exhalation and absorption of their natural moisture into a dry hard sort of mummy, incapable of any farther change from the agency of those causes, to which, in such situations, they are exposed.

preserved by frost and exclusion of moisture.

Similar causes produce the same effects on wood. Even under less rigid circumstances of this kind, as in the roofs and other timber of large buildings, it continues for an astonishing length of time unchanged; witness the timber of that noble edifice Westminster Hall, built by Richard II in 1397; and the more extraordinary instance quoted by Dr. Darwin, in his ingenious work the Phytologia, of the gates of the old St. Peter's church in Rome, which were said to have continued without rotting from the time of the emperor Constantine to that of pope Eugene IV, a period of eleven hundred years. On the other hand, wood will remain for ages with little change, when continually immersed in water, or even when deeply buried in the earth; as in the piles and buttresses of bridges, and in various morasses. These latter facts seem to show, that, if the access of atmospherical air is not necessary to the decay of wood, it is, at least, highly conducive to it.

Timber long preserved in large buildings,

under water, and in earth.

In posts fixed in the ground and exposed to the weather, we constantly find that part soonest decay, which is just above

Decays soonest at the surface of the ground,

or where moisture may lodge.

above or within the ground. So also where there is an accidental hole in an exposed surface, or any artificial cavity, as in a mortice and tenon, or the part where pales nearly touch the rails on which they are nailed, there the wood universally begins first to moulder away. The same thing happens with regard to horizontal rails themselves, which, when made of the same materials, rot much sooner than the pales which they support. These facts are very easily explained. They clearly show, that the great cause of decay is the constant action of water aided by air, which most affects those points, where it is most retained, but has less operation, where, as in the perpendicular pales, it chiefly runs off by its own gravity, so that the little which remains is easily and quickly abstracted by the cooperating power of the sun and wind.

This owing to putrefaction.

The change which I am describing is the consequence of putrefactive fermentation; a chemical operation, in which the component parts of the wood form new combinations among themselves, and with the water which is essential to the process. The precise nature of these new compounds has not been ascertained; but, so far as they are known, they consist of certain gasses, or species of air, which fly off, and leave behind a powder, consisting chiefly of carbon or charcoal, and the earth which entered into the original composition of the wood.

Water acts mechanically also.

Beside this chemical change depending on water, that substance tends to destroy wood exposed to the open air by a mechanical operation. Every farmer is acquainted with the power of winter in mouldering down the earth of his fallows. It is equally well known, that porous freestone splits and shivers during severe winters. These effects are produced by frost, which, acting on the water in the pores or interstices of these substances, expands it by conversion into ice, and thus bursts the minute cells in which it was contained. There can be no doubt, that a similar operation takes place to a certain extent in exposed wood, and thus in some degree promotes its destruction.

Water and air the chief instruments,

It appears, then, that the contact of water and air are the chief causes of the decay of wood. If, therefore, any means can be devised, by which the access of moisture and

air

air can be prevented, the wood is so far secure against decay. This principle may be illustrated by supposing a cylinder of dry wood to be placed in a glass tube or case, which it exactly fills, and the two ends of which are, as it is called, hermetically sealed, that is, entirely closed by uniting the melted sides of each end of the tube. Who will doubt that such a piece of wood might remain in the open air a thousand years unchanged? Or let us take a still more apposite illustration of this fact; that of amber, a native bitumen, or resin, in which a variety of small flies, filaments of vegetables, and others of the most fragile substances are seen imbedded, having been preserved from decay much longer probably than a thousand years, and with no apparent tendency to change for ten times that period. Let us see then if we cannot, by the exclusion of moisture and air, find means of virtually placing our timber in a case of glass or amber.

therefore to be excluded.

Thus amber preserves insects, &c.

With this view, various expedients have been employed, of which the most common is covering the surface with paint; which is oil mixed with some substance capable of giving it the colour which we desire. It is well known, that several of the oils, as those of linseed, hempseed, &c., become dry when thinly spread on any hard substance. The drying quality is much assisted by their being previously boiled with certain metallic oxides, more especially that of lead, litharge. The crust so formed is with difficulty penetrated by moisture or air. For this purpose drying oil is spread on silk or linen, in the manufacture of umbrellas; and will tolerably well succeed in confining hidrogen gas, or inflammable air, in the construction of air-balloons. Hence we see the mode in which the application of paint on wood serves to defend it against the causes of destruction.

Paint employed with this view.

When paint is employed within doors, it is customary to add to the oil, beside the colouring matter, some essential oil of turpentine, which not only makes it dry more readily, but, by giving it greater tenuity, causes it to flow more freely from the brush, and therefore to go farther in the work. For the same purposes I observe it forms a part of the paint used on wood and iron work in the open air; but, as it appears to me, most improperly: For I have re-

Uses of oil of turpentine in it.

Its disadvantages.

marked

marked that on rubbing wood painted white, and long exposed to the weather, the white lead has come off in a dry powder like whiting; as if the vehicle which glued it to the wood had been decomposed and lost, leaving only the pigment behind: And I have been much inclined to suspect, that this has arisen from the oil having been too much *opened*, as the workmen call it, or having its thickness and tenacity too much diminished by a superabundance of the oil of turpentine. In this state it may, in various ways, be more readily acted on by water and air. We know, that the properties of what are called unctuous or fat oils are much changed by the admixture of the volatile or essential oils. On this principle we succeed in getting grease out of woollen cloths by oil of turpentine; but whether the same change is produced on the drying oils, I have not learned.

Acts similarly in discharging grease.

Is the pigment of use?

It appears, then, that these drying oils either by themselves, or boiled with metallic oxides, will form a varnish on wood; but it may be questioned how far the colouring matters, with which they are usually mixed, contribute to increase their preservative power. I do not however, deny, that they may be serviceable in this and other views. They might be supposed to enable the oil to lay firmer hold, as it were, on the wood; and they may serve to increase the thickness of the defensive covering. The first of these points is of some importance; for we observe that the paint on street doors, which is become thick by frequent incrustation, is apt, from the strong influence of the summer's sun, to separate from the polished wood beneath, and rise in large blisters; probably in consequence of a greater expansion in the crust itself than in the subjacent wood. Here, therefore, the colouring matter of the paint fails to produce the desired effect; and as to the second end, or that of increasing the thickness of the covering, that may, probably, be much more effectually accomplished than by the mere addition of pigments, some of which are capable of chemical decomposition, and all are costly. This purpose an ingenious artist has of late attempted to answer, by recommending an admixture of road-dust; and for that and other means of reducing the price of paints, has obtained a premium

This doubtful.

Road dust.

a premium from the London Society of Arts*. However just the general principle in this case may be, the application is somewhat unphilosophical; unless it shall be found, which will scarcely be admitted, that dust of every chemical and mechanical quality will equally or sufficiently answer the intended purpose.

Some material of this kind, selected with greater precision, may however undoubtedly be useful; and none I think promises more fairly than siliceous or flinty sand, which, so far as we know, is absolutely indestructible, and which may be easily procured from the sea-shore, and from the currents of the clear rivers and roads in Berkshire and other counties abounding with siliceous stones. Sand from the sea must first be cleared from all saline impregnations by washing in several waters; and any sand may be obtained of the fineness desired, by mixing it with water in a tub, and after having stirred the whole well together, pouring out, in a longer or shorter time, the muddy water, from which the sand will settle by its own gravity, in a state fit for use when dried. Perhaps fine sand preferable.

More than thirty years ago this subject presented itself to my mind, on seeing some water-shoots, which had been pitched and painted in the common way, taken down in a state of complete rottenness. I had read that charcoal, buried in the moist earth, had come down to us perfectly sound from the times of the Romans; and that posts long withstood the same moisture, if the part intended to be put into the ground was charred all round to a certain depth. Impressed with these facts, I determined to try an artificial coat of charcoal; and when new water-shoots were constructed, I strongly and carefully rubbed them with a coat of drying oil, which I immediately dredged all over with a thick layer of charcoal finely powdered, and contained in a muslin bag. After two or three days, when the oil was thoroughly dried, and firmly retained the greatest part of the charcoal, I brushed off what was loose, and over that which adhered I applied a coat of common lead-coloured paint, and a few days after, a second. The Water-shoots. Covered with drying oil, and this dusted with charcoal.

* See Journal, Vol. XIV. p. 258.

whole

whole became a firm and solid crust; after which the shoots were put into their places, and being examined many years afterward, appeared perfectly sound. Any other colour would probably have succeeded equally well with that which I employed. I do not think that lamp black, which is a pure species of charcoal, would have answered the purpose of forming a thick defensive covering so well as the grosser charcoal which I used. But whatever sort of charcoal is employed, it ought either to be fresh made, or heated again in close vessels, so as to expel the water which it greedily attracts from the air.

Lamp black perhaps not so good.

Drying oils expensive.

To all compositions formed from drying vegetable oils there is this objection; that however well they may answer the end proposed, they are too dear for that great consumption, which is usually required for outside work. For this and other reasons, various other substances have been employed for the same purpose.

Pitch does not answer.

Of these the most common is pitch, which is well known to be the resinous matter melted by heat out of the pine tribe of trees in form of tar, and afterward hardened by evaporation. It is applied hot, and when cold, makes a moderately hard varnish. It does not however appear, in fact, to answer the purpose so well as might have been expected. The sun at first melts it, so that it runs off in drops, or adheres to every thing which touches it; and the united influence of air and water seems to make it brittle and powdery like resin. Experience therefore shows it to be of little value. Neither is it probable that its powers would be much improved by admixture with charcoal, sand, or other similar substances. Many members of this Society may recollect its application twenty years ago on the red-deal shingled roofs of part of our market. In this case it was used hot, mixed with Spanish brown, and hardened by sand sifted over it with a sieve; notwithstanding which it seems to have left the wood like the unmixed pitch, and, though frequently renewed, has not prevented the necessity of various repairs within these last five years. The original boards are now every where more or less in a state of decay.

The

Coal tar.

The bituminous substance melted by heat out of coal, and commonly called coal tar, has been strongly recommended for this purpose by that ingenious philosopher Lord Dundonald. I have tried it largely and unsuccessfully, though perhaps not fairly; for the workman whom I employed, in order to make it work more easily, added to it oil of turpentine, which certainly diminished its durability by rendering it more miscible with water. I am however inclined to believe, that no substance of this kind, used by itself, will become sufficiently dry and hard to resist the influence of the weather.

Animal oils made drying.

As animal oils are considerably cheaper than those expressed from vegetables, attempts have been made to communicate to them a drying quality. This has been effected by dissolving in them while hot various substances capable of being melted, in such a portion that the whole mass would become dry and hard when cold. Bees' wax, resin, and brimstone are found to have this property. Some of them, when united with drying oil, have long been employed for making boots and shoes water-proof, or impervious to moisture*. But they will also succeed when mixed with train oil, which is obtained from the blubber of the whale. In the second volume of the Memoirs of this Society, printed in the year 1783, there is the following receipt. "Melt twelve ounces of resin in an iron pot or kettle; add three gallons of train oil and three or four rolls of brimstone; and when the resin and brimstone are melted and become thin, add as much Spanish brown, or red or yellow ochre, or any colour you want, first ground fine with some of the oil, as will give the whole as deep a

Composition of this sort.

Old receipt for water-proof boots or shoes.

* For this purpose there is the following receipt by Mr. Barker in Sir John Hawkins's edition of that entertaining work, Isaac Walton's complete Angler: 4th edition, page 223. "Take a pint of linseed oil, with half a pound of mutton suet, six or eight ounces of bees' wax, and half a pennyworth of resin. Boil all this in a pipkin together; so let it cool till it be milk-warm. Then take a little hair-brush, and lay it on your new boots; but it is best that this stuff be laid on before the boot-maker makes the boots; then brush them once over (with it) after they come from him. As for old boots, you must lay it on when your boots be dry."

4 shade

shade as you like. Then lay it on with a brush as hot and thin as you can. Some days after the first coat is dried, give it a second. It will preserve plank for ages, and keep the weather from driving through brick-work." Page 114.

Tried with apparent success.

This composition I tried about eighteen years ago on some elm paling, substituting for the colouring matter one or two coats of common white paint for the sake of the appearance. This paling appears to me to be in every part of it, which was so covered, as sound as when it was first put up.

Bees wax added.

As compositions of the resinous kind are apt to crack and become powdery, like the varnish of carriages, by exposure to weather, it is not improbable, that this effect may be in some measure counteracted by the mixture of a small proportion of bees' wax. Such a compound I have used, but in the quantity of eight ounces to the gallon found it too slow in drying, and capable of being easily scraped off with the nail. Wax is also at this time very scarce and dear*.

Remarks.

All the substances contained in these mixtures are capable of perfect incorporation with each other by heat, and when separately exposed, are with great difficulty acted on by water or air in any heat which occurs in our climate.

Method of application.

They should be applied hot with a common painter's brush on the wood which is previously very dry, so as to sink deeply into its pores; and though at first they are apparently somewhat greasy when cold, yet after some days they make a firm varnish, which does not come off on rubbing. When it is required to give beauty to the work, colouring matters may either be added to the mixture, or afterward applied over it in form of common paint. Two

* For the information of those who may be inclined to make a trial of these compositions, I have inquired the wholesale prices of the different ingredients of Messrs. Cave and Co. Bristol, from whom I learn, that they are very fluctuating, train oil being from 2s. 3d. to 3s. 2d. per gallon; resin from 12 to 21 shillings per cwt.; roll brimstone from 34 to 38 shillings per cwt.; and bees' wax from 3s. 3d. to 3s. 6d. per lb.; the lowest of these prices being about what these articles at present bear.

coats

coats of the composition should always be given; and in all compound machinery, the separate parts should be so varnished before they are put together; after which it will be prudent to give a third coating to the joints, or to any other part which is peculiarly exposed to the action of moisture, such as water-shoots, flood-gates, the beds of carts, the tops of posts and rails, and all timber which is near or within the ground. Each coat should be dry before the parts are joined, or the last coat applied.

These compositions are equally efficacious in keeping iron from decay by rusting. They might also be very advantageously employed in rendering water-tight the plaster, which is used to case the outside of the arches of vaults unsheltered by roofs, provided the mortar were made perfectly dry, and the covering of the arch brought up to an angle, instead of making it follow the form of the arch in an elipse or the segment of a circle. It would preserve iron, and render arches impervious to water.

It is necessary to mention, that compositions made of hot oil should for the sake of security be heated in metallic or glazed earthen vessels in the open air. For whenever oil is brought to the boiling point, or 600° of Fahrenheit's thermometer, the vapour immediately catches fire, although not in contact with any flame; and though a lower degree of temperature than that of boiling should be used in this process, it is not always practicable either exactly to regulate the heat, or to prevent the overflowing of the materials, in either of which cases, were the melting performed in a house, the most fatal accidents might follow. Caution.

The following is the proportion of the above ingredients, and the mode of mixing them, which I should recommend.

Take 12 ounces of resin, and 8 ounces of roll brimstone, each coarsely powdered, and 3 gallons of train-oil. Heat them slowly, gradually adding 4 ounces of bees'-wax, cut into small bits. Frequently stir the liquor, which, as soon as the solid ingredients are dissolved, will be fit for use. What remains unused will become solid on cooling, and may be remelted on subsequent occasions. Mode of making the composition.

Charcoal powder or sand may be added.

If the addition of charcoal powder or siliceous sand contributes to the durability of drying oil, it may probably have a similar effect on this composition; but whether it may be best to mix them with the ingredients, or apply them afterward, I cannot from experience tell. In the latter case, the powder should be sifted on, while the first coat of the composition is still hot; and, after some days, when that is dry, should have a brush gently passed over it, in order to remove all the particles which do not adhere; after which other coats of the composition may be applied, as before directed.

This is all which occurs to me as to the mode of preserving wood when exposed to the weather.

(To be concluded in our next.)

III.

On the Blight in Wheat. By Mr. THOMAS DAVIS, *of Horningsham* *.

Wheat blight a plant.

THE opinion I gave in the Bath Society's Papers, Vol. X, p. 41, that the wheat blight is a *plant*, and not an insect, is now fully confirmed by the microscopical observations of that able naturalist, Sir Joseph Banks, who, in his treatise on the subject, has given magnified representations of the plant, in which its form and fructification are so conspicuous, that no one can doubt the fact †.

Sir Joseph also describes the manner in which the minute seeds of this plant (which are as light as air) are carried by the wind, and lodged on the growing stalks of wheat, where they take root and vegetate, and, like all other parasitical plants, rob the plant to which they attach of its nourishment, to support themselves. The effect is too well known. The rapidity with which these minute plants vegetate, and the destruction they make in a crop of wheat, of which the ears only a few days before

Its destructive operation.

* From the Bath Society's Papers, Vol. XI, p. 111.

† See Philos. Journal, Vol. X, p. 225, and Plates IX, X.

appeared

appeared full and heavy and nearly fit for the sickle, can scarcely be believed by those who have not observed it, and is astonishing even to those who have watched its progress. It seems to produce something more than a mere cessation of growth. *Its action is like that of poison.* It absorbs the *farina* or flour of the fairest and plumpest grain, and reduces it to a mere shell of bran.

Remedy difficult.

But although the nature of this disease is now so well known, the remedy is not so easily found. With all due deference to the great abilities of Sir Joseph Banks, I am not so sanguine as to expect, that it can be eradicated by pulling up the diseased plants: or even, if it were practicable, by burning all the straw of every blighted crop.

The plant not confined to wheat.

The seeds of this destructive plant are too minute and abundant, and capable of being wafted to too great a distance, to be totally destroyed. A single acre of blighted wheat will produce seed enough to supply a whole district; and indeed it is too well known to botanists, that the plant grows and flourishes on many *other plants* beside wheat. And were there but a single piece of wheat in a country where none had grown before, the enemy would be ready for the attack, whenever there was a *predisposing* cause in the wheat crop to receive it.

Predisposing cause to be guarded against.

It is probably not within the power of man to prevent, *totally*, the ravages of this destructive, though minute enemy to agriculture, but it may yet be in his power to reduce them in a considerable degree, by ascertaining and obviating the *causes which peculiarly dispose and prepare the wheat plant* for its attacks. These may be summed up in one word, viz. *weakness*, or *debility*.

This debility.

The class of plants called by botanists mosses and lichens are the *insects of the vegetable kingdom, created to prey on weak plants, as the insects of the animal kingdom, are to prey on weak animals.* In both instances, the juices by being weakened and deprived of their acridity become *their* proper food. The *remedy* must be to restore to the object *its natural health and vigour.*

Mosses and lichens analogous to insects.

To apply this argument to wheat, and to show the causes which render it unproductive, it will be necessary to con-

sider the nature of the plant, and the kind of cultivation which usually renders it productive.

Mode in which wheat grows.

It is well known, that nature has furnished the wheat plant with a double set of roots, so contrived, that the first may be deep enough to enable it to stand the severity of the winter; and the second so shallow as to admit the genial influence of the spring. It first shoots down a perpendicular *tap-root*, which supports the plant and keeps it *steady* during the winter; and in the spring it tillers out a number of *coronal shoots*, *each* of which has its *own proper root*, and produces its *own ear*, though still adhering to and dependent on each other for mutual support; and when that operation is complete, the winter root *becomes useless* and *dies*.

If this winter root be imperfect, the side shoots which are to produce the crop will also be so. A strong solid *foothold* for the tap-root is therefore necessary for wheat; and the more complete the winter root is, before the spring tillering takes place, the more perfect will be the crop. If the formation of the young plants be unequal, so will be the ripening of the crop; and if the ripe ears on one part of the plant are waiting for the green ones on the other, the blight generally attacks the crop.

An irregularly ripening crop subject to blight.

Thin and late crops particularly so.

A *thin* crop of wheat, and a *late ripening* crop, (and a thin crop is usually a late ripening one,) are the peculiar *prey of the blight;* and these are generally produced either by sowing land with wheat, which is unfit for wheat, or in an improper state of cultivation, or by sowing it in an improper season. *In fine, any cause which tends to weaken the plant, will predispose it to receive the blight.*

Causes that render wheat weak.

The causes which tend to weaken the wheat plant are many, but the following are the most obvious:

1*st*. Sowing wheat on land that has been so worn out by cropping, as to have lost that *tenacity* and *cohesion*, which are so necessary to a wheat crop, and which even dung, *without rest*, will not restore.

2*dly*. Sowing the land in a light loose state, whereby the wheat plant roots too near the surface, and is liable to be injured by the winter's frost, and to have its roots laid bare by the wind.

3*dly*.

3dly. Sowing wheat too late in the autumn, (which is too common,) especially in poor land and exposed situations, where the roots have not time to establish themselves before the winter comes on, and vegetation is totally at a stand.

Now as these causes have, in consequence of the advance in the price of wheat, occurred more frequently of late years than formerly; it is probable that the assertion "that the blight on wheat has increased of late years" may be true. For, Probably increased of late.

1st. It has not been uncommon to sow land with wheat every *third* year, instead of every fourth or fifth: and as the land, in the interim, has been under crops, the very nature of which is to make land light, and no fallow year having been allowed to get it close again; the crops, though abundant in straw, have not had strength enough to support them till harvest, and have been *laid by the rains*, and thereby become a prey to the blight. From sowing wheat too frequently,

2d. It has been very much the practice of late years to sow wheat after *turnips*, and very *clean* crops have been produced thereby. But this system is wrong: the turnips are eaten before they are wanted, and the wheat is sown a month too late; and being necessarily *late ripe*, is often attacked by the blight. or after turnips,

3d. It has been also a frequent practice to sow wheat *after potatoes*; and this system is still worse: the land is rendered too light for wheat, and the seed time is much too late, unless it be in deep rich land, where the wheat plants will grow during the whole of the winter. or after potatoes,

4th. And even the practice of sowing *wheat after clover* has been carried to too great an extent on light land, especially where the land is nearly tired of clover. It encourages the *slug*, and the *wire-worm*, which destroy a considerable part of the wheat plants, leaving the residue a *thin unequal crop*, which the blight seldom fails to attack, and frequently to ruin. and in some cases clover.

To sum up the whole:—If it can be proved, (and every man *who is a farmer* must have observed it,) that all *weak crops* of wheat, and particularly all *late-ripe crops*, are peculiarly subject to blight; it should be the great object of General directions.

of every farmer to sow such land, *and such only*, to wheat, as is fit for wheat; to get it in order *early* in the summer, that it may be *close* and *firm* before sowing; to sow as *early* as the state of the weather will permit, particularly in cold soils or exposed situations; and to sow *those kinds of wheat*, which are disposed to *ripen* early, (a circumstance much more attended to in Scotland than in England;) but above all, not to wear out his land by cropping it *oftener with wheat* than its nature will bear; always considering, that it is *not the number of acres sown*, but *the number of bushels produced*, that will *enrich the farmer*, *or supply the market*.

Blight in some cases increased by manure.

When I assert that *weak* crops are the most susceptible of blight, I do not altogether mean such crops as are weak *in consequence of a want of manure*, but such as grow on land which has been made so light by repeated culture, that the plants cannot get firm *foothold*, the great *desideratum*, in fact the *sine quâ non* of a good wheat crop; and manure, particularly horse-dung, instead of remedying this defect, only adds to the evil. In this instance, the remark which has been often made, that the highest manured crops are the most susceptible of blight, is perfectly consonant with my observation. For the same reason, these crops are apt to fall before they are ripe, and in that situation if there be any blight in the air, they are sure to be infected with it, because the sun cannot dry them, and the circulation of the sap is impeded by the bruising of the straw.

Too much manure injurious.

It was well observed in one of the Agricultural Reports, "that land may be made so drunk with dung, that a wheat crop cannot stand upon it:" and I will defy any man to get a good *yielding* crop of wheat in a highly-manured garden. He may, and probably will, get a good crop of *straw*.

Wheat on certain lands seldom blighted.

Mr. A. Young is right, in saying in his Annals, that on *high* land, *not of the best quality*, wheat is seldom blighted. The reason is, that such land is not made too loose by culture and manure, and the straw stands upright and exposed to the sun and wind. I had a very striking instance of this on the Marquis of Bath's land, under my care, a few years ago: I had ploughed up twenty acres of furze-

Instances of blight on land loosened too much by cul-

furze-land in the autumn, with intent to sow it to wheat. It was run back in the spring, and cross-ploughed early in the summer, so as to be quite close and firm before wheat sowing; but having occasion to plant two acres of potatoes, I took part of this land and manured it well with rotten dung, and planted the potatoes therein. They were ripe early, and when dug, the two acres were sown with wheat; on the same day, the rest of the piece, which had not been dunged at all, was sown. The wheat on the two acres was much the proudest during the winter, and the best crop when it came into ear; but when it was just ripe, (which was ten days after the other part,) the blight struck it, and it was as black as a coal, while the rest was as bright as silver. In fact the two acres were scarcely worth reaping. ture and manure.

Again, with respect to *late-ripening* crops being subject to the blight, I am of opinion, that the act of ripening wheat and all annual graminiferous plants is not so much an effort of nature, as a *cessation of nature's efforts;* and that no crop of grain can be a good one, unless the whole ripens together; and if by any cause, particularly by the seed being sown too thin, or by a partial failure of the plants from a severe winter, the plant is forming new roots, or one part of it is doing so, while the other is or ought to be ripening its seed, the straw keeps green and moist, instead of turning yellow and dry, and the blight is sure to take it. And this has brought drilling into disgrace more than all other causes, particularly when the crop has been sown too thin, or the hoe has been used too late *.

Ripening a cessation of action.

Circumstances tending to bring drilling into disrepute.

* I have just been a witness to the threshing a piece of drilled wheat, which was injured by harrowing in grass seeds in April: the harrowing made the wheat too thin, and caused it to throw out new shoots; it kept growing while it ought to have been ripening; it of course took the blight, and though the ears were six inches long, the produce weighed only 40lb. per bushel.

IV. *Answer*

IV.

Answer to Remarks on a Pamphlet, lately published by the Rev. T. VINCE, *respecting the Cause of Gravitation. In a Letter from the Author.*

To Mr. NICHOLSON.

SIR,

MY *Observations on the Cause of Gravitation* having been attacked in the last number of your Journal, by a person calling himself *Dytiscus*, you will, I trust, do me the justice to admit my answer in the next.

Object of Mr. Vince's pamphlet.

What I proposed to establish was, that the fluid assumed by *Sir J. Newton* as the cause of gravitation cannot produce a force to impel a body towards the sun, which shall vary as $\frac{1}{a^2}$. The principal objection is to the 18th article, in which it is said, " hence, according to the foregoing reasoning, taking only the two first terms of the series, &c. &c." In the foregoing reasoning we took a^m for the density of the medium, and then the force was represented by the sum of the alternate terms of the Binomial theorem; in this particular case, therefore, we take the two first terms only, as is here proposed. But in the present article we represented the density by $Pa^m+Qa^q+Ra^r+$, &c. each of which terms gives a series for the force, similar to that stated above; here, therefore, according to the same proceeding, we take the two first terms of each of these series. This *must* be the meaning of the words, " taking only the two first terms of the series;" for they must mean, either the two first terms of each of the series composing the whole force, or the two first terms of the whole considered as one series. But the latter meaning would have entirely excluded all the other series for the force, arising from the general law of density $Pa^m+Qa^q+Ra^r+$, &c. continued to an *indefinite* number of terms, and which it was the declared intention of the proposition to take in, and here confined the force to *two* terms only, Pa^m+Qa^q. It would, therefore, have been totally inconsistent with the terms of the proposition, to have taken the

The principal objection founded on a mistake.

the words in the latter sense, as it would have destroyed it as a *general* proposition here proposed for investigation, and reduced it to a *particular* case. Besides, the two first terms of the whole as forming one series would not have had a *definite* meaning; for we might write down all the first terms of each series, and then all the second terms, in their order: or, we might write down the first and second terms of the first series, and then the first and second terms of the second series, and so on. As the terms of each series are the alternate terms of the binomial theorem, the first terms only of the first three series were put down, with +, &c. showing that the other terms were to be understood. Two terms only of each series were proposed to be taken, to show, even upon that supposition, that as different powers of *a* must then enter into the two first terms of each series. The whole could not constitute a quantity which should vary as $\frac{1}{a^2}$. But Mr. D. seems to have paid no attention to the words, "taking only the two first terms of the series," nor to have considered that the word *series* is used in both numbers; and hence, instead of taking the first and second terms of each series, he has taken the first terms of the two first series, leaving out the second terms which it was proposed to take in, and thus (to use his own expression) "committed so palpable a blunder," as totally to do away the whole force of his objection.

But Mr. D. goes on thus: "on this point the professor's whole demonstration rests." This is another most unfortunate mistake, and contains a further proof, how little Mr. D. attended to the subject. What is here assumed, is only a very near approximation to the force, but still sufficient for our purpose; the correct law of the force is obtained by taking in *all* the terms of each of the series, instead of the two first only. And the proof of my proposition further rests upon this, that the density of the planet enters into the expression for the force; on which account it was not necessary for me to have gone any further in the investigation than the 13th article; here I have fully established my proposition: but the subject being

Another mistake pointed out.

new,

new, and of a curious nature, I was induced to consider it a little further.

The objection proves nothing against the proposition.

If, however, Mr. D's objection had been well founded, it would have proved nothing against my proposition, as his own conclusion shows, that the force does not vary as $\frac{1}{a^2}$, the density e entering into the expression for the force. Upon his own assumption, taking in *all* the terms of the series, the force will be represented under this form; $\frac{\alpha}{a^2}+\frac{\beta}{a^4}+\frac{\gamma}{a^6}+$ &c. and can this vary as $\frac{1}{a^2}$, even omitting e?

Farther examination of the objection.

But there is another ground upon which we may examine the objection. The quantity $Pa^m+Qa^q+Ra^r+$, &c. representing the density of the fluid, must always be *positive;* hence P, Q, R, &c. m, q, r, &c. are under certain restrictions, such as to make the above quantity positive for every value of a. Now we must have some standard for our quantities. Let, therefore, the sun's radius$=1$, the density of the fluid at the sun's surface$=1$. Now according to *Sir J. Newton's* hypothesis, the fluid pervades the sun, causing thereby the gravitation within as well as without the sun. Also, a varies from o to infinity. Now according to Mr. D's assumption of Q, m, q, the density of the fluid is represented by $P-\frac{Q}{a^2}$; when, therefore, by diminishing a, $\frac{Q}{a^2}$ becomes greater than P, the density of the fluid becomes *negative*, that is, there is no fluid, and consequently no gravitation. Make $P-\frac{Q}{a^2}=o$, and $a=\sqrt{\frac{Q}{P}}$. From the centre of the sun, therefore, to this distance, there is no fluid: hence, according to Mr. D's assumption, part of the sun is not endued with gravitation! he has therefore made an *illegal* assumption of the quantities Q, m, q; what then becomes of his objection?

The second objection answered.

But he has brought forward another objection. He says, I ought to have used $3m$, $3q$, for $2m$, $2q$; this, he asserts *would* have been the case, *if* I had estimated the density

density of the fluid as *Newton* did, that is, by the quantity of matter in a *given cubical space*. True; but the nature of my proposition necessarily obliged me to measure the density by the quantity of matter on a *given plane;* my $2m$, $2q$, are therefore perfectly correct, and this is no new use of the term *density;* it is so used when we say the density of light, heat, &c. varies inversely as the square of the distance. With so little attention did Mr. D. examine my investigation, as not to see, that I was under the necessity of so estimating it on the ground I took; for he imputes it to a mistake, that I did not estimate it as *Newton* did. Is not this a "palpable blunder?".

From an attentive consideration of what is advanced by Mr. D., I am clearly of opinion, that he did not read the whole of the essay, so as to comprehend the true grounds upon which the truth of my proposition rests. He seems only to have looked amongst the expressions, to see, if by assuming particular values of the quantities, he could not prove against the proposition; imagining that such values were unlimited, and altogether misunderstanding the subject. If I am wrong in this conjecture, his mistakes must have arisen from his not having mathematical knowledge sufficient to comprehend the investigation.

The objector did not examine the whole of the investigation.

The truth of my proposition rests upon two independent circumstances—that the density e of the planet enters into the law of force; and that by taking in all the terms of the series expressing the force, it is impossible to make the force vary as $\frac{1}{a^2}$, even omitting e. What then becomes of Mr. D's vaunting assertion, "on this point the Professor's whole demonstration rests." From the scientific knowledge displayed by Mr. D. in his animadversions on my essay, we are justified in applying to himself his own words, *mutatis mutandis;* "the errours in the works of Dytiscus afford no very flattering specimen of the mathematical abilities of this country."

He further objects thus: "what he says respecting the interference of the ethereal atmosphere of the different planets is totally foreign to the question." Not totally foreign, for it makes directly against the existence of such an

What was said of the ethereal atmospheres of the planets an argument against their existence.

an atmosphere, so far as we have any experience of elastic fluids.

The reviewers vindicated.

Mr. D. is very angry with the Reviewers, that they were not so quick-sighted as himself, in discovering the faults in my essay; what has here been said may, perhaps, tend a little to explain the reason.

The information, that my paper might probably not be printed, came from the Secretary, *Dr. Grey;* this Mr. D. acknowledges was not prudent conduct. But I conceive myself to have been also very uncivilly treated on this account, that the Secretary, whose duty it was to have informed me how my paper was disposed of, never communicated to me the information. Having had occasion to mention *Sir J. Pringle*, and not having been aware of any circumstances, which ought to have prevented me from stating my opinion of his character, I thought it proper to pay him a mark of respect, justly due to his memory.

I am, Sir,
Your obedient Servant,
S. VINCE.

Cambridge,
April 6th, 1808.

V.

Observations on the Structure of the different Cavities, which constitute the Stomach of the Whale, compared with those of ruminating Animals, with a View to ascertain the Situation of the digestive Organ. By Everard Home, *Esq. F. R. S.**

Object of the author.

THE following observations are in some measure a continuation of those upon the stomachs of ruminating animals contained in a former paper. They are intended to show that the stomach of the whale forms a link in the gradation toward the stomachs of truly carnivorous animals.

* Abridged from the Philos. Trans. for 1807, Part I, p. 93.

The

The number of cavities constituting the stomach are not the same in all animals of the whale tribe. In the common porpoise, grampus, and piked whale, the number is the same as in the bottle-nose porpoise; but in the bottle-nose whale of Dale there are two more cavities. This variation is however by no means material, since the general structure of the stomach is the same. Stomachs of the whale tribe.

In all of the whale tribe there is one cavity lined with a cuticle, as in the bullock and camel. First cavity.

In all of them there is a second cavity made up of a very glandular structure. In the porpoise, grampus, and large bottle-nose whale this structure resembles that which is above described. In the piked whale the rugæ are longitudinal and deep, but in some places united by cross bands; and as the piked whale has whalebone teeth, the great whalebone whale will probably, from the analogy of its teeth, resemble it in the structure of its stomach. Second cavity.

The third cavity in all of them is very small, and bears a strong resemblance to the third cavity in the camel's stomach; its use, therefore, is probably the same. Third cavity.

The fourth stomach in all of them has a smooth internal surface, with the orifices of glands opening into its cavity. In the bottle-nose whale of Dale the two additional cavities have the same internal structure, and therefore must have the same general use, with a greater extension of surface, and the subdivisions will make the food pass more slowly into the intestine. Fourth cavity.

The first stomach of the whale is not only a reservoir, but the food undergoes a considerable change in it. The flesh is entirely separated from the bones in this cavity, which proves that the secretion from the glandular part has a solvent power. This was found to be the case in the bottle-nose porpoise and large bottle-nose whale. In both of them several handfuls of bones were found in the first stomach, without the smallest remains of the fish, to which they belonged. The soft parts only can be conveyed into the second and third stomachs, the orifices being too small to admit the bones to pass. Office of the first stomach.

The bones must therefore be reduced to a jelly in the first stomach, and although the process, by which this is effected,

effected, being slower than that, which separates the flesh, is the reason of their being found in such quantity in the cavity, the means by which it is performed are probably the same.

Mr. Hunter's opinion of the second cavity.

The second cavity was supposed by Mr. Hunter to be the true digesting stomach, in which the food becomes chyle, and the use of the third and fourth he looked upon as not exactly ascertained*.

Erroneous.

Upon what ground Mr. Hunter was led to draw this conclusion cannot now be ascertained; and, ſuch is my respect for his opinion, that nothing but the following observations, supported by facts, could lead me to form a different one. In considering this subject, it struck me that the second stomach could not be that, in which chyle is formed, since that process having been completed, any other cavities would be superfluous. The last cavity in all stomachs is that, in which the process must be brought to perfection: and therefore the most essential change, which the food undergoes, or that by which it is formed into chyle, should be performed in that cavity. Surveying the different cavities in the whale's and ruminating stomachs with this impression on my mind, and comparing them with the single stomachs of carnivorous animals, it appeared that the first point, which required to be ascertained, was, which of the cavities in these more complex stomachs bears the greatest resemblance to the simple one. The fourth of the whale is certainly more like the human stomach than the second or third. I therefore concluded, that the fourth, both from analogy and situation, is the stomach in which the process is completed: and that in this animal, from the peculiarities of its œconomy, and the nature of the food, not only a cuticular stomach is necessary, but also two glandular ones, in which it undergoes changes preparatory to its being converted into chyle.

The chyle always formed in the last cavity.

Compared with the stomach of the camel;

Having satisfied myself upon this subject, and having compared the stomachs of the whale with the fourth of the camel, the contraction or partial division of the camel's made it apparent, that the lower portion only of that ca-

* *Vide* Observations on the Structure and Œconomy of Whales. By John Hunter. Philos. Trans. Vol. LXXVII, p. 411.

 vity,

vity, which resembles in shape and internal appearance the human stomach, is the cavity in which chyle is formed, and the upper or plicated portion is only to prepare the food, and is therefore analogous to the second in the whale.

As the same appearances are met with in the fourth stomach of the bullock, as well as in the camel, although there is no permanent contraction, or division between them, the upper or plicated portion must be considered as a preparatory organ, and the lower portion as that, in which the formation of chyle is completed. This receives farther confirmation from a more attentive examination of the parts immediately after death, by which it was found, that, before the stomach has been disturbed, there is an evident muscular contraction between the plicated and lower portion. This appearance was met with in every instance that was examined, and these were not fewer than nine or ten. Added to this the lower portion, on a more minute inspection, has an appearance somewhat similar to the inner membrane of the human stomach: and the surface of the plicæ is in many respects different. and of the bullock.

From the facts and observations which have been stated, it appears, that, in many animals of the class mammalia, the food undergoes different changes preparatory to its being converted into chyle, and this last process is effected by a somewhat similar secretion, since the part of the stomach which produces it has in all of them an evident similarity of structure. Chyle produced by a similar secretion in all the mammalia.

The above facts appear to throw some light on the digestion of the different kinds of food, and open a wide field of inquiry into one of the most interesting parts of the animal œconomy, which has been hitherto too much neglected. In the present very limited state of our knowledge there are many circumstances, which cannot be accounted for: these however will be explained, when a further progress has been made in this investigation.

It is obvious, that as the stomachs of carnivorous animals are the most simple, animal substances, on which they feed, require a shorter process to convert them inte chyle than vegetables; but why the whale tribe, which live on fish, Animal substances easier converted into chyle than vegetables.

fish, should have a more complex stomach, it is not easy to explain: since fish are very readily converted into chyle, in the stomachs of animals of their own class, as well as in the human stomach, and there is therefore reason to believe, that they require as little preparation for that process, if not less than animal substances.

Ruminating animals capable of living on fish, without injury from their bones.

The fish bones swallowed by the whale tribe being retained in the cuticular bag, till they are reduced to jelly, explains the circumstance of cows, and other ruminating animals being able occasionally to live on fish, (a fact, of which there is no doubt, both in the Orkneys and in Iceland;) since, if the bones are dissolved in the paunch, the other stomachs are in no danger of being injured from the animal living on this kind of food.

Whether these cavities, which I have called preparatory stomachs, are solely for purposes connected with digestion, or are also in any way connected with the formation of secretions peculiar to those animals, cannot be ascertained in the present state of our knowledge of digestion.

Anomaly of the spermaceti whale.

The oil of the physeter, which crystallizes into spermaceti, shows some affinity in this respect to the secretion of fat, that becomes suet, which is only met with in ruminating animals: but on the other hand, the oil of the rest of the whale tribe does not form this substance, more than the fat of the horse produces tallow. These facts may be hereafter explained by an examination of the digestive organs of the physeter, when an anatomist shall have an opportunity of examining them.

VI. On

VI.

On Family Wine Making. *By* W. Matthews, *Esq.* *.

To the Committee of Superintendance of the Bath and West of England Society.

Gentlemen,

Having in the 10th volume of the Society's papers been indulged with the insertion of a few remarks on the utility of making family wines from several of our garden fruits; I took the liberty of presenting, at a subsequent General Meeting, for its examination, a sample of such wine made under my own notice. It will be within the recollection of different gentlemen, who attended that meeting, that the wine they tasted was deemed a very good, pleasant-flavoured, and useful article. The price at which it was made † was considered as small, when compared with the uses to which the wine may be applied, even in genteel families, where economy is regarded. But the idea of making such an article, in considerable quantities, (especially in abundant fruit years,) so as to have the power of furnishing sick and sickly poor persons with such occasional refreshment, could not pass unapproved. The oldest wine of this sort which I now have by me, is yet too young to give proof of that excellence, which three or four years more will give it; but it is now so rich and valuable, that I can have no hesitation about publishing the recipe, by which it is made, and encouraging any of our members fully to rely upon it for success. The fruits used were of the different sorts mentioned in the recipe, excepting gooseberries, and I think nearly of equal quantities, taken out of a private garden, where they would otherwise have turned to very little account. My friends having fully convinced me, that if I gave them white wine equally good

Home made wine,

for family use,

and the sick poor.

Goodness.

* Papers of the Bath and West of England Society, vol. XI, p. 222.

† This will be from 2s. 6d. to 3s. per gallon, according to circumstances.

with that produced, they will not call on me for foreign white wine, of at least *five* times the price; I have this year taken the advantage of a fine fruit season, and made several hogsheads. If I live to present the Society with a taste of it some years hence, I have no doubt of its being found worthy of their commendation.

Several hogsheads made.

Black currants recommended.

I cannot conclude without repeating my recommendation to the owners of gardens in general, to all farmers in easy circumstances, and country gentlemen especially, to regard this useful practice:—and that they may do it to the greater advantage, the increased cultivation of the black-currant plant seems essential: It is easy to increase, greatly productive, and its fruit, in general, can scarcely form too large a proportion of the mixture.

I remain, with all due respect,
Your faithful coadjutor,
WILLIAM MATTHEWS.

Bath, September, 1807.

A useful Recipe for making Family Wine.

Receipt for the wine.

Take, black currants, red ditto, white ditto, ripe cherries, (black hearts are the best) rasberries, each an equal, or nearly an equal quantity: If the black currants be the most abundant, so much the better.—To 4lb. of the mixed fruit, well bruised, put one gallon of clear soft water: steep three days and nights, in open vessels, frequently stirring up the mass: then strain through a hair sieve. The remaining pulp press to dryness. Put both liquids together, and to each gallon of the whole put three pounds of good, rich, moist sugar, of a bright yellowish appearance. Let the whole stand again three days and nights, frequently stirring up as before, after skimming off the top. Then tun it into casks, and let it remain, full and purging at the bung-hole, about two weeks. Lastly, to every nine gallons put one quart of good brandy, and bung down. If it does not soon drop fine, a steeping of isinglass may be introduced, and stirred into the liquid, in the proportion of about half an ounce to nine gallons.

N. B.

N. B. Gooseberries, especially the largest, rich flavoured, may be used in the mixture to great advantage; but it has been found the best way to prepare them separately, by more powerful bruising, or pounding, so as to form the proper consistence in pulp; and by putting six quarts of fruit to one gallon of water, pouring on the water at twice, the smaller quantity at night, and the larger the next morning.—This process, finished as aforesaid, will make excellent wine, unmixed; but this fluid, added to the former mixture, will sometimes improve the compound. Gooseberries may be added.

ANNOTATION.

I am inclined to think the addition of brandy, here recommended, injurious: an opinion founded on the authority of a respected friend, formerly a chemist in a country town, who excelled in making family wine, and confirmed by my own experience. A similar sentiment is entertained by Dr. Anderson, as appears in his judicious letter on the subject to the author of the preceding article, inserted in Vol. X of the Bath Society's papers, which I shall here annex. Brandy perhaps better omitted.

I will only add, that the best home made wine I recollect to have tasted was made by expressing the juice of white currants, bruised but not picked from the stalks: adding water to the fruit after it was pressed, in the proportion of double the quantity of juice: mixing the two liquors together, and putting the whole into a barrel with three pounds of pretty coarse brown sugar to every gallon of the mixture: stirring it well, and then leaving it to ferment with the bunghole at first open, and afterward loosely covered, the barrel not being quite filled. As the sugar does not immediately dissolve, the stirring must be repeated occasionally at intervals of a few days, till this is effected. After it has fermented properly, the barrel must be stopped close; and it may afterward be bottled for use. Some useful information respecting the fermentation and management of wine may be obtained from Mr. German's paper on the wines of Champagne; Philos. Journal, Vol. XVII, p. 353. Method of making white currant wine.

 Isleworth,

Isleworth, Jan. 24, 1804.

DEAR SIR,

"I received your letter some days ago respecting the *wines* that may be made from the natural fruits of this country, which I should have sooner answered, could I communicate any thing of the importance I wished; but that not being the case, I felt a great reluctance at the thought of troubling you with any thing not satisfactory.

Our own fruits afford wine as good as foreign.

"I can say little else than that from our own experience for a short time past, and what I have seen of others, I am perfectly satisfied that wine may be made from our native fruits—red and white currants, gooseberries, black currants, rasberries, and other fruits, (with the help of sugar) as good, and of as rich a flavour in all respects, as any that are imported from abroad. But the particulars in the process that may vary the qualities of the wine, where the materials are the same, are so numerous, and the time that must elapse before the result of any experiment can be known is so great, that I despair of living to see any certainty established on this head. At present I sometimes taste as good wine of that sort as could be desired, and again as bad as can be thought of, made by the same persons, when they can assign no reason for the difference. From our own limited practice I have been able to ascertain only two points, that I think can be relied upon as tolerably well etablished. These are, *first*, that age, I mean not less than *three* years, is required to elapse, before any wine, that is to be really good, can attain such excellence as to deserve the name of *good;* and *second*, that it never can attain that perfection, if spirits of any kind be mixed with it. I apprehend that most of our made wines are greatly hurt by not adverting to these two circumstances.

Liable to fail.

Two leading points.

Quality of the fruit.

"Another circumstance that is in my opinion very necessary for the formation of good wine of this sort, is a certain degree of *acidity* in the fruit, without which the wine never acquires the zest which constitutes its peculiar excellence, but hurries forward too rapidly into the state of vinegar. Currants at all times possess enough of that acidity; but if gooseberries be too ripe they are apt to want

want it, and become insipidly sweet at an early period; though they soon become vinegar. It ought to be remarked, that the native acidity of the fruit is different from the acidity of vinegar, and possesses qualities extremely dissimilar. The sourness of vinegar, when it has once begun to be formed, continues to augment with age; but the native vegetable acid, when combined with saccharine matter, is gradually diminished as the fermentation proceeds, till it is totally lost in the vinous zest into which both this and the sugar are completely converted before any vinegar is produced: if the fermentation be properly conducted. Acidity of fruit is not vinegar.

"This I believe is a new opinion, which experience alone enabled me to adopt not very long ago. But I have had so many experimental proofs of *this* fact, independent of the support it derives from reasoning, that I am satisfied it is well founded. I am satisfied farther, that the wines of this country are debased chiefly by not adverting to it, and of which I think you will be convinced also by a moderate degree of attention.

"Every person knows, that an insipid sweetness is the prevailing taste in liquors when they begin to ferment, and that it is gradually changed into a pungent vinosity as the process proceeds; but few persons have had occasion to remark, that the *native acid* of fruit undergoes a similar change by the fermentatory process. Every one who tastes made wines, however, soon after the process has commenced, perceives that sour to a certain degree is mixed with the sweet. It chances, indeed, that the sweet is sooner blended than the sour; so that when the liquor is tasted a few months after it has been made, it hath lost some part of its sweetness; but still retains nearly the whole of the sourness of the native acid of the fruit. And as the vinous flavour is yet but weak, the liquor appears to be thin and weak, and running into acidity. It is therefore feared, that if it be not then drunk, it will soon run to the state of vinegar; on this account it is often used in this state, when it forms a very insipid beverage. Frequently also, with a view to check the acetous process, and to give that degree of strength which will entitle it to the name of a cordial The sweetness goes off, and so would the acidity. Common mistake.

liquor,

Brandy, injurious in its effect.

liquor, a certain portion of brandy is added to it, after which it may be kept for some time. The effect of this addition is to put a stop to that salutary process of fermentation which was going slowly forward, and gradually maturing the native vegetable acid into vinous liquor, which being at last blended with the saccharine vinous juice, produces that warm exhilarating fluid which cheers the heart, and invigorates the strength of man. In this way the sharp insipid and poor liquor which was first tasted is, by a slow process, which requires a great length of time to complete it, converted into rich pleasant wine, possessing, in a great degree, that high zest which constitutes its principal excellence.

"My experience does not yet enable me to speak with certainty respecting all the circumstances that may affect the flavour, or augment or diminish the strength of wine, or accelerate or retard the time of its ripening. But my opinion at present is, that a great part of the flavour of wine depends considerably upon the skin of the fruit, which may be augmented or diminished by the degree of pressure the fruit is subjected to, and other particulars connected with it; or by the macerating the fruit more or less in the juice before the skins be separated from the pulp: and that the ultimate qualities of the wine are considerably affected by the proportion of the original native acid of the fruit, conjoined with the saccharine part of the juice. It seems to me very evident also, that the saccharine juice can be more quickly brought into the state of wine than the acid portion of it, and that of course those wines that consist entirely of saccharine matter, flavoured only by some pleasing vegetable perfume, such as cowslip or elder-flower wine, and others of similar sorts, may be sooner brought to be fit for drinking than those in which the juices of fruit form a considerable ingredient; and may be also made of a weaker and lighter quality. And that fruit-wines, in proportion to the diminution of the quantity of fruit to that of sugar, or in proportion to the quantity of acid in the fruit, may be accelerated or retarded in the progress of fermentation; but that strong full-bodied wine, of good flavour, must have a considerable proportion of native acid, and

The flavour affected by the skin of the fruit.

Some sooner fit for drinking than others.

and requires to be kept a long while before it can attain its ultimate perfection.

"I have had too little experience in the practice of making grape wine to enable me to speak with precision. The flavour of different kinds of grapes we know varies considerably, which must affect the wine; but other circumstances in the process must affect it greatly. It is the only fruit known in this country that affords juice in abundance sufficient to admit of being made into wine without the addition of water, or rich enough without the use of sugar. Two years ago the season was so favourable, that my grapes (the muscadine) ripened completely, and I determined to try to make some wine of them without either sugar or water. The juice was squeezed out by hand without any pressure, as I had no press. It fermented very well, and after a proper time it was tried. The liquor tasted sweetish, but wanted much of the vinous zest we wished for. This arose, I have no doubt, from the want of a due proportion of native acid, which would have been probably supplied by a complete pressure of the must, had I possessed the means of doing it, especially if the bunches of grapes had not been separated from the small foot-stalks to which the berries adhere. But not having a quantity sufficient to make it worth while to have a press, I thought of another method of attaining the end I aimed at, to which I was forced to resort; on finding that birds and vermin are so greedy of the grape, that it is a matter next to impossible to preserve them for any time here in quantities after they are ripe without being broken, which, by letting the juice flow out, lodges between the berries in the clusters, and there becomes mouldy, and communicates a musty taste that cannot be got rid of.

Grape wine.

Birds and vermin fond of grapes.

and frequently occasion a musty taste by opening them.

"To avoid all these evils, I determined to gather the fruit when it is so far ripened only as just to begin to be pecked by the birds. As the juice possesses at that time more vegetable acidity, and less of the saccharine taste than when fully ripe, I conceive that the wine made from it will be sharper, and have a higher zest than the other; but dreading that the juice might not be sufficiently matured to do by itself, I added a portion of sugar and water to the juice, and

Attempt to remedy this.

and have put it by for trial. It fermented well, and the liquor has at present as promising an appearance as I could wish. Should this mode of making grape wine succeed, it will be by far the cheapest wine we can make in this country; for the quantity of juice yielded by the grape is so much more abundant, and so much richer than that of our other fruits, and it is so much easier to be gathered and otherwise managed, that it must be much more desirable. The quantity of fruit produced too is much greater when the vines are properly managed, than can be gotten from the same extent of ground of other fruits, as to give it a decided preference on the whole. I have just now in my cellar above forty gallons of that wine made from the grapes that were gathered from a wall of about fifteen yards in length, and fifteen feet high. Nor was that crop above the average. Neither had that wine above half the quantity of sugar that other fruit wines would have required. I have no doubt that were vines raised from seeds of the best and earliest sorts, and carefully selected when they come to bear, we might thus obtain a grape that would ripen very well in this country without the assistance of a wall. It is by no means improbable that such a vine was once known in England.

Advantages of the grape.

Black currant ranks next.

"Next to the vine, I agree with you in thinking that the *black* currant is the best fruit we have of that kind for making wine. I have seen some of it that was truly excellent. It would be of great use for giving flavour to some other wines.

"When I began this letter I thought that I had nothing to say; but being once begun, it has run on to an enormous length. I hope you will forgive me for it. I now speak little, and write less: and it requires an effort for me to begin with either; but, like a disorderly clock, when I am once fairly set agoing, I run on perhaps without rhime or reason. Wishing you success in all your useful pursuits.

"I remain, dear Sir,

"Your most humble servant,

"JAMES ANDERSON."

VII. *Description*

VII.

Description of the Mineral Bason in the Counties of Monmouth, Glamorgan, Brecon, Carmarthen, and Pembroke. By Mr. EDWARD MARTIN. *Communicated by the Right Hon.* C. F. GRENVILLE, *F. R. S.**

1 THE irregular oval line, delineated on the annexed map (Plate IX.) shows nearly the inner edge of a limestone bason, in which all the strata of coal and iron ore (commonly called iron stone) in South Wales are deposited; the length of this bason is upwards of 100 miles, and the average breadth in the counties of Monmouth, Glamorgan, Carmarthen, and part of Brecon, is from 18 to 20 miles, and in Pembrokeshire only from 3 to 5 miles. Limestone basin containing all the strata of coal and iron ore in South Wales.

2. On the north side of a line, that may be drawn in an east and west direction, ranging nearly through the middle of this bason, all the strata rise gradually northward; and on the south side of this line they rise southward, till they come to the surface, except at the east end, which is in the vicinity of Pontipool, where they rise eastward. On the north of the centre the strata rise to the north, and on the south to the south.

3. The depths from the surface to the various strata of coal and iron ore depend upon their respective local situations. Depths from the surface vary.

4. The deepest part of the bason is between Neath, in Glamorganshire, and Llanelly, in Carmarthenshire; the uppermost stratum of coal here does not extend a mile in a north and south direction, and not many miles in an east and west direction, and its utmost depth is not above 50 or 60 fathoms. Deepest part of the basin. Uppermost stratum.

5. The next stratum of coal, and those likewise beneath it, lie deeper and expand still longer and wider, and the lowest which are attended by parallel strata of iron ore, of which there are in some situations about 16 accompanied by irregular balls or lumps of iron ore, occupy the whole space between Llanmaddock Hill, near the entrance of Burry river, to Llanbidie, from the Mumbles to Cribbath, from Newton Down to Penderryn, from Castle Coch to Castle Morlais, and from Risca to Llangattock, and in Second and lower strata.

* From the Philos. Trans. for 1806, p. 342.

length

length on the south side of the bason from Pontypool through Risca, Tinkwood, Llantrissent, Margam, Swansea Bay, and Cline Wood, to Llanmaddock Hill, and on the north side through Blaenafon, Ebbw, Sirhowy, Merthyr, Aberdare, Aberpergwm, Glyntowy, Llandibie, and the Great Mountain, to Pembrey Hill, near Llanelly in Carmarthenshire, and their depths are at the centre range of strata from 6 to 700 fathoms.

Strata running through Carmarthen bay and Pembrokeshire.

6. The strata of coal and iron ore running from Pembrey Hill, through Carmarthen Bay and Pembrokeshire to St. Bride's Bay, are only a continuation of those in the counties of Glamorgan and Carmarthen, which lie next to and parallel with the north side of the bason, all the remaining strata rising southward; and the middle ranges on the north side of the bason, are lost between where they meet the sea near Llanmaddock Hill and the south side of Pembrey Hill, in their course towards Pembrokeshire, in consequence of a contraction of the sides of the mineral bason, or rather by its becoming shallower; for in Pembrokeshire none of the strata of coal or iron ore lie above 80 or 100 fathoms deep, consequently all those which do not lie above 5 or 600 fathoms in Glamorganshire and Carmarthenshire have not reached this county, by reason of the bason not being of sufficient depth and width to hold them.

Strata at the east end of the basin.

7. The strata of coal at the east end of the bason running from Pontypool to Blaenafon and Clydach, and on the north side from thence to Nanty Glo, Ebbw, Beaufort, Sirhowy, Tredegar, Romney, Dowlais, Penderryn, Plymouth, Cyfarthfa, Abernant, Aberdare and Hurwain Furnaces and Iron Works, are of a cokeing quality, and thence the whole strata of coal to St. Bride's Bay alter in their quality to what is called stone coal, (the large of which has hitherto been used for the purposes of drying malt and hops, and the small, which is called culm, for burning of limestone); the several strata of coal from Pontypool, on the south side of the bason, through Risca, Llantrissent, Margam, and Cline Wood, to Burry River, Llanelly, and the south side of Pembrey Hill, are principally of a bituminous or binding quality.

8. Notwithstanding

8. Notwithstanding the principal strata of coal in Glamorganshire lie from 5 fathoms to 6 or 700 fathoms deep, still it has not been necessary to pursue these strata deeper than about 80 fathoms. Strata worked only to eighty fathoms.

9. The veins of coal and iron ore, in the vicinity of most of the iron works in Monmouthshire and Glamorganshire, are drained and worked by levels or horizontal drifts, for which opportunity is given by the deep valleys which generally run in a north and south direction, intersecting the range of coal and iron ore, which run in an east and west direction, under the high mountains, and thereby serving as main drains, so that the collier or miner here gets at the treasures of the earth, without going to the expense and labour of sinking deep pits, and erecting powerful fire-engines. However, in process of time, in situations where the coal and iron ore that are above the level of these natural drains become exhausted, it will be found necessary to sink shallow pits, and erect fire-engines for the draining and working of the coal and iron ore, and at a future period, pits of greater depths must be sunk for the same purposes. Method of working.

10. There are 12 veins or strata of coal in this mineral depository, from 3 to 9 feet thick each; which together make 70½ feet: and there are 11 more, from 18 inches to 3 feet, which make 24½ feet, making in all 95 feet; beside a number of smaller veins from 12 to 18 inches, and from 6 to 12 inches in thickness, not calculated upon. Number and thickness of coal veins.

11. By taking the average length and breadth of the foregoing different strata of coal, the amount is about 1000 square miles, containing 95 feet of coal in 23 distinct strata, which will produce in the common way of working 100,000 tons *per* acre, 64,000,000 tons *per* square mile. Produce in the common way of working.

12. If the whole extent of this mineral country was an even plain, the border or outbreak of each stratum would appear regular and true; but owing to the interposition of hills and valleys, the edges of the strata, if nicely measured and planned, would seem indented and uneven, yet in many instances the due range is totally thrown out of course, in consequence of knots, dikes, or faults. Edges of the strata disturbed.

13. These

The irregularities extend far into the basin.

13. These faults or irregularities are not confined to the edges of the strata, but they take grand ranges, through the interior of the bason, generally in a north and south direction, and often throw the whole of the strata, for hundreds of acres together, 40, 60, 80, or 100 fathoms, up or down, and still there is seldom any superficial appearance, that indicates a disjunction, for the largest faults frequently lie under even surfaces.

It is not probable, that any vein or stratum remains undiscovered.

14. As every stratum rises regularly from its base to the surface, and is frequently visible and bare, in precipices and deep dingles, and often discovered where the earth or soil is shallow in trenching, or in forming high roads, and by reason of the whole of the country within this boundary being so perforated by pits, and so intersected by the various operations of art and nature, it is not probable that any vein of coal, iron ore, or other stratum remains undiscovered in this mineral bason.

Their distribution among the counties.

15. Glamorganshire engrosses far the greatest portion of coal and iron ore, Monmouthshire the next in point of quantity, Carmarthenshire the next, Pembrokeshire the next, and Brecknockshire possesses the least.

Breakings out of the strata in Brecknockshire.

16. The strata of coal and iron ore in the last named county, which are the lowest in the bason, break out northward, and only take place in the three following distinct spots, *viz.* 1st. From Turch River (which is the boundary between Lord Cawdor and Charles Morgan Esq.) across the river Tawe and the Drin Mountain to the great forest of Brecon. 2d. A corner of ground from Blaen Romney to the north of Brynoer. 3d. Another spot, from Rhyd Ebbw and Beaufort Iron Works, through Llwyn y Pwll, near Tavern Maed Sur, to where it joins Lord Abergavenny's mineral property.

Principal faults.

17. *Note.* A principal fault is observable at Cribbath, where the beds or strata of the limestone stand erect, another, of considerable magnitude, lies between Ystradvellte and Penderryn, where all the strata on the north side of the bason are moved many hundreds of yards southward (as at Dinas).

Limestone.

18. *Note.* The limestone appears to the surface all along the boundary line in the counties of Monmouth, Glamorgan, Carmarthen,

Carmarthen, Brecon, and no doubt can be entertained of its due range from Newton across Swansea Bay to the Mumbles, and from Llanmaddock Hill across Carmarthen Bay to Tenby. In Pembrokeshire it appears to the surface on the south side of the bason, at Tenby, Ivy Tower, Cochelard, Bit Church, Williamston, Lawrinny, Cord, Canta, and Johnston; and on the north side of the bason, at Templeton, Picton, Harriston, and Persfield; yet it certainly forms an underground connection from point to point.

The following is an Enumeration of the Strata, as they appear in the Section, at the Foot of Plate IX.

Enumeration of the strata.

1. $1\frac{1}{2}$ foot Cwm little vein.
2. $2\frac{1}{2}$ feet Hendro Vawr vein.
3. Three or four small veins of coal.
4. 3 feet the yard vein of Cwm.
5. $1\frac{1}{2}$ Do. the little coal vein.
   2 or 3 courses of regular balls are seen between 5 and 6.
6. 4 feet Cwm Canaid coal.
   Between 6 and 7 are balls not yet worked.
7. 4 feet, Clynderris coal.
   The division between 7 and 8 varies much in the perpendicular distance between the veins sometimes 30 and sometimes 20 yards.
8. 4 feet, the clay vein.
9. 9 feet, Cwm Glo big vein.
   Balls and little veins of mine are seen in the division between 9 and 10.
10. 9 feet, Cwm Whern big vein.
11. 2 feet, Cwm Glo little vein and $1\frac{1}{2}$ foot little vein with $\frac{1}{2}$ yard of rubbish between them.
    2 or 3 poor little veins of mine occur between 11 and 12.
12. 1 foot vein above the balls.
    2 courses of balls, but no veins, between 12 and 13.
13. 4 feet, Whern vein, a little rubbish in the middle.
    There are mines in the division between 13 and 14; but not yet worked.
14. $2\frac{1}{2}$ feet, and 3 feet vein. These appear at Penywain with 1 foot rubbish between them.

16. 3 feet

Enumeration of the strata.

15. 3 feet, Dowlais little vein, at Penywain.
No mine yet found in the division between 15 and 16.

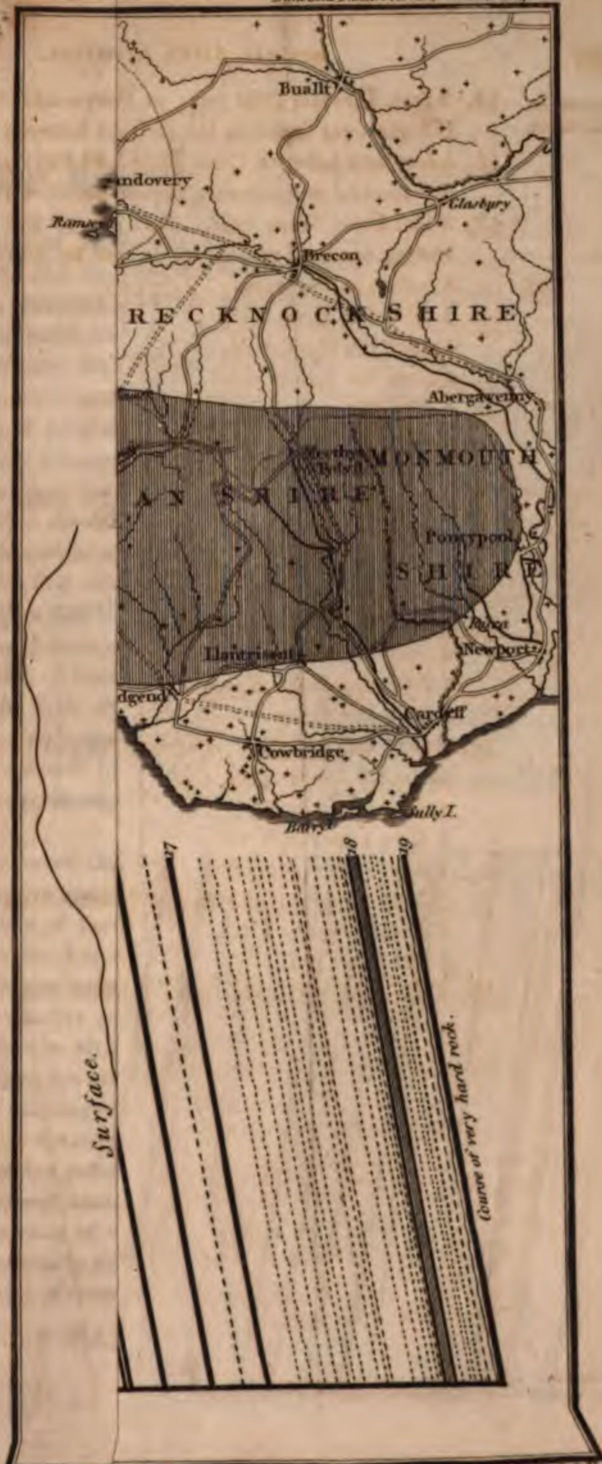
16. 4 feet vein between Cwm Moin and Penywain.
No mine of consequence occurs between 16 and 17.

17. 3 feet Cwm Moin vein. Between 17 and 18 the following occur in succession as here set down.
3 inches yellow vein.
3 ditto Pin Brith.
4 ditto the black vein.
4 ditto the yellow vein.
4 ditto the jack vein.
2 ditto the Gurthean vein.
5 ditto the Gurthean Clase Vawr.
5 ditto the Gurthean Clase, or blue vein.
1 ditto upper black pin.
2 ditto lower black pin.
4 ditto the big vein.
3 ditto Gurthean Spinkin.
4 ditto Gurthean Vawr gons.
2 ditto Gurthean Knappe.
3 ditto Pin Garw.

18. Smoot and fire clay. Between 18 and 19 are
4½ inches lower black vein.
4 ditto black balls.
1 ditto upper inch vein.
1 ditto lower inch vein.
2 ditto upper 2 inch vein and 2 inches lower 2 inch vein.
2 ditto irregular balls.
3 ditto best pin.

19. Course of very hard rock, 3 feet.

VII. *On*


Buallt
ndovery
Glasbury
Brecon
RECKNOCK SHIRE
Abergavenny
MONMOUTH
SHIRE
AN SHIRE
Pontypool
Newport
Llantrisant
dgend
Cardiff
Cowbridge
Barry
Sully I.
17
18
19
Surface.
Course of very hard rock.

VIII.

On Fairy-Rings. By W. H. Wollaston, *M. D. Sec. R. S.**

THE circles of dark-green grass frequently observed in old pastures, and known to most persons by the name of Fairy-rings, although in themselves of no importance, yet seem to claim some attention, if we consider the many ingenious attempts that have been made to explain their origin. On such a subject I shall be excused offering any examination of opinions previously formed by others, and shall therefore proceed briefly to relate such observations as I made, during a few years residence in the country, on the progressive changes of these circles, and which seem to me to lead to a clear and satisfactory conclusion. Various attempts to account for fairy rings.

That which first attracted my notice, was the position of certain fungi which are always to be found growing upon these circles, if examined in a proper season. In the case of mushrooms, I found them to be solely at the exterior margin of the dark ring of grass. The breadth of the ring in that instance, measured from them toward the centre, was about twelve or fourteen inches, while the mushrooms themselves covered an exterior ring about four or five inches broad. Certain fungi grow about them.

The position of these mushrooms led me to conjecture that progressive increase, from a central point, was the probable mode of formation of the ring. I was the more inclined to this hypothesis, when I found that a second species of fungus presented a similar arrangement, with respect to the relative position of the ring and fungi; for I observed, that in all instances the present appearance of fungi was upon the exterior border of a dark ring of grass. I thought it not improbable that the soil, which had once contributed to the support of fungi, might be so exhausted of some peculiar *pabulum* necessary for their production, as to be rendered incapable of producing a second crop of that singular class of vegetables. The second year's crop would consequently appear in a small ring surrounding the original centre of vegetation, and at every succeeding year These occasion the ring by spreading progressively from the centre, as they cannot continue to grow in the same spot.

* Phil. Trans. for 1807, p. 133.

the

the defect of nutriment on one side would necessarily cause the new roots to extend themselves solely in the opposite direction, and would occasion the circle of fungi continually to proceed by annual enlargement from the centre outwards. An appearance of luxuriance of the grass would follow as a natural consequence, as the soil of an interior circle would always be enriched by the decayed roots of fungi of the preceding year's growth.

Dr. Hutton's observation of them at Arthur's seat.

By reference to Dr. Hutton's* "Observations on certain natural appearances of the ground of the hill of Arthur's Seat near Edinburgh," we find the progressive enlargement distinctly noticed; but as he happened not to observe any of the fungi that occasioned them, he speaks of it merely as "a piece of natural history worth recording, and for which, a theory is wanting."

Respecting the enlargement, he says, "from all the observations I have made, this progress seems always to have proceeded in the direction of a line bisecting the segment, that is to say, those portions of concentric circles are never inscribed, but always circumscribed; and for this reason it appears, that those circles of which segments are exhibited to our observation must be increasing and not diminishing in their diameters."

Dr. Withering ascribed them to their true cause.

Although Dr. Hutton has overlooked the real origin of these appearances, Dr. Withering has ascribed them to their true cause; but his remarks are confined to one species of agaric (the *ag. orcades* of his Arrangement), and do not appear to have been confirmed by any subsequent observation of their annual progress.

"I am satisfied," says he, "that the bare and brown, or highly clothed and verdant circles in pasture fields called Fairy-rings are caused by the growth of this agaric." "Where the ring is brown and almost bare, by digging up the soil to the depth of about two inches, the spawn of the fungus will be found of a grayish white colour; but where the grass has again grown green and rank, I have never found any of the spawn existing."

* Edinburgh Transactions.

Had

Had Dr. Withering frequently repeated this examination of the soil he would have corrected the last remark, which is not universally true, as the grass may at some period be found luxuriant even over the undecayed spawn. During the growth of the fungi, they so entirely absorb all nutriment from the soil beneath, that the herbage is for a while destroyed, and a ring appears bare of grass surrounding the dark ring. If a transverse section be made of the soil beneath the ring at this time, the part beneath the fungi appears paler than the soil on either side of it, but that which is beneath the interior circle of dark grass is found on the contrary, to be considerably darker than the general surrounding soil. But in the course of a few weeks after the fungi have ceased to appear, the soil where they stood grows darker, and the grass soon vegetates again with peculiar vigour; so that I have seen the surface covered with dark grass, although the darkened soil has not exceeded half an inch in thickness, while that beneath has continued white with spawn for about two inches in depth.

Spawn of fungi sometimes found under the luxuriant grass.

The section of the space occupied by the white spawn has in general nearly the same form, and may be compared to that of a wave proceeding from the centre outwards, as its boundary on the inner side ascends obliquely toward the surface, while its exterior termination is nearly in a vertical position. The extent occupied by the spawn varies considerably according to the season of the year, being greatest after the fungi have come to perfection, and is reduced to its smallest dimensions, and may in some cases not be discernible, before the next year's crop begins to make its appearance.

Progressive course of the spawn.

For the purpose of observing the progress of various circles I marked them three or four years in succession, by incisions of different forms, by which I could distinguish clearly the successive annual increase, and I found it to vary in different circles from eight inches to as much as two feet. The broadest rings that I have seen were those of the common mushroom *(ag. campestris)*; the narrowest are the most frequent, and are those of the champignon *(ag. orcades* of Dr. Withering). The mushroom accordingly makes circles of largest diameter, but those of the

Annual increase of the circle various.

Broadest when from the common mushroom: narrowest from the champignon.

Three other species have the same effect.

champignon are most regular. There are, however, as many as three other fungi that exhibit the same mode of extension, and produce the same effect upon the herbage. These are the *ag. terreus*, *ag. procerus*, and the *lycoperdon bovista*, the last of which is far more common than the two last mentioned agarics.

Confirmation of this cause.

There is one circumstance that may frequently be observed respecting these circles, which can satisfactorily be accounted for, according to the preceding hypothesis of the cause of their increase, and may be considered as a confirmation of its truth. Whenever two adjacent circles are found to interfere, they not only do not cross each other, but both circles are invariably obliterated between the points of contact: at least in more than twenty cases, I have seen no one instance to the contrary. The exhaustion occasioned by each obstructs the progress of the other, and both are starved.

Different fungi stop the progress of each other.

I think it also not unworthy of observation, that different species of fungi appear to require the same nutriment; for in a case of interference between one circle of puff-balls and another of mushrooms, they did not intersect; but I cannot say positively that I have seen more than one instance.

Circle interrupted by a tree.

I once found that a tree had interrupted the regular progress of a circle; but this appeared to be only a temporary impediment, as the extension had proceeded at the usual rate, and by passing obliquely from each side into the soil beyond the tree, had given the ring the form of a kidney, so that another year or two would probably reunite the two extremities into one curve surrounding the tree.

The spawn will not vegetate again on the spot for some time.

Being desirous of ascertaining in what length of time a soil might again recover the power of producing a fresh crop of fungi, I cut a groove, in one or two instances, along the diameter of a mushroom-ring, and inserted a quantity of spawn taken from its circumference, with the hope of seeing it vegetate for some distance near the centre; but the experiment failed altogether: and as I shortly after quitted my residence in the country, I had no opportunity of repeating the experiment, and must leave it to be prosecuted by those who are more favourably circumstanced.

IX. *Account*

IX.

Account of a Musical Instrument, called an Organized Lyre, invented by Mr. Adolphus Ledhuy, *late Geometrical Surveyor of Forests, of Coucy-le-Château, in the Department of the Aisne*.*

THE object of the author was simply to improve the guitar-lyre, but by a simple mechanism he has rendered the sounds of this new instrument susceptible of several different tones or stops, by means of which the performer may imitate several instruments, such as the lyre, the piano forte, the harp, &c.; while at the same time it is as easy to play upon as the guitar-lyre, being fingered in the same manner, and not more inconvenient for carriage. In accompaniments, soloes, and quartettoes, or with several other instruments, it answers equally well: and, when it was submitted to the examination of the first artists in Paris, the inventor received the most flattering encomiums. **Organized lyre.** **Capable of imitating different instruments.**

Mr. Adolphus has likewise composed instructions for his new lyre, in which he details every particular necessary for learning to play on it without a master: and in a second part he has added examples and lessons of every kind, to point out the advantages derivable from his invention in gradations of tone and expression; so that any one, who plays already on the guitar, or lyre-guitar, may render himself familiarly acquainted with this instrument in less than a month.

The following is a description of the instrument. **Description of the instrument.**

1. The organized lyre has fifteen strings, separated into three distinct divisions, and embracing the compass of four complete octaves. The three divisions are called the base, tenor, and treble. **Fifteen strings in three divisions.**

2. It has a row of six keys, which include the extent of three octaves. With these the pianoforte may be imitated, but the sounds produced are more soft. **Keys.**

* Sonninis Bibliothèque Physico-économique, July, 1807, p. 61. The inventor has taken out a patent for this instrument in France.

Mute.

3. By means of a mute the performer may change the sound of the instrument, either gradually or instantaneously, from the loudest of which it is capable to the softest, or the contrary.

Mode of applying the mute.

To apply this mute the performer has not the least occasion to employ his hand, or stop his performance: all that is required is to press with his arm on a pedal, which is precisely at the place where the arm rests habitually on the instrument, and to increase or diminish this pressure, till the mute produces the desired effect.

Two necks for fingering.

4. The instrument has two necks, each with six strings, which are fingered in the same manner as the guitar-lyre.

Case.

5. The case of the instrument, which is indispensably necessary for its conveyance from place to place, is equally so for playing on it; because, the performer being obliged to have the left knee raised a little, the better to support the instrument, and to give freedom of movement to the arm, he rests his foot on the box, out of which rises a stand for the music, which may be raised or lowered at pleasure. This stand folds up so as not to increase the size of the case, and adds but little to its weight.

X.

A Botanical and Economical Account of Bassia Butyracea, or the East India Butter Tree. By W. ROXBURGH, *M. D.**

BASSIA BUTYRACEA.

Polyandria monogynia.

Generic character.

CALYX beneath, four or five leaved. Corol, one petalled: border about eight cleft. Berry superior, with from one to five seeds.

* *Bassia butyracea.* Roxburgh.

Specific character.

Calyx five-leaved; stamens thirty or forty, crowning the subcylindric tube of the corol.

* From the Asiatic Researches, Vol. VIII.

Fulwah,

Fulwah, *phulwarah*, or *phulwara*, of the inhabitants of the *Almorah* hills, where the tree is indigenous. Flowering time, in its native soil, the month of *January;* seeds ripe in *August*. Synonimes.

Trunk of the larger trees, straight, and about five or six feet in circumference. Bark of the young branches smooth, brown, and marked with small ash-coloured specks. Trunk.

Leaves alternate, about the ends of the branchlets, petioled, obovate-cuneate, obtuse-pointed, entire; smooth above, villous underneath; veins simple, and parallel; length, six to twelve inches; breadth, three to six. Leaves.

Petioles, from one to two inches long. Petioles.

Stipules, if any, minute, and caducous. Stipules.

Flowers numerous, round the base of the young shoots, and from the axils of the lower leaves, peduncled, large, pale-yellow, drooping. Flowers.

Calyx, four, five, or six leaved (five is by far the most common number); ovate, obtuse, covered externally with ferruginous pubescence, permanent. Calyx.

Corol; tube subcylindric, length of the calyx; border of eight, spreading, oblong, obtuse divisions, longer than the tube. Corolla.

Stamens; filaments from thirty to forty, about as long as the tube of the corol, and inserted on its mouth. Anthers linear-oblong. Stamens.

Pistil, germe conical, (ten or twelve celled, one seeded,) downy, surrounded with a downy nectarial ring. Style longer than the stamens; stigma acute. Pistil.

Berry oblong, generally pointed by a remaining portion of the style; smooth, fleshy, containing one, two, or three, rarely more, large seeds; the rest not ripened. Berry.

Seeds oblong, rather round than flat, but differing in shape according to the number contained in each fruit; smooth, shining, light brown, with a long, lanceolate, lighter coloured, less smooth, umbilical mark on the inside. Seeds.

This tree, which is rendered interesting on account of its seeds, yielding a firm butyraceous substance, resembles Resembles bassia latifolia.

bassia latifolia, (see *Coromandel* Plants, Volume I, No. 19; also *Asiatic* Researches, Volume I, page 300,) so much as scarce to be distinguished from it, except by the corol and stamina.

Difference in the corols,

Here (in *bassia butyracea)* the corol is of a thin texture, with a tube nearly cylindric, and border of eight, large, spreading, oblong segments. There (in *bassia latifolia)* it is thick and fleshy, with a gibbous, indeed almost globular tube; and border of generally more than eight, small, cordate, rather incurved segments.

and stamina.

Here, the stamina, from thirty to forty in number, have long filaments inserted *on* the mouth of the tube of the corol. There they are fewer in number; have very short filaments, and are arranged in two, or three series, completely *within* the tube, to which they are affixed.

Other species.

It may not be improper to notice here some other species of the same genus. The following Botanical description of *bassia longifolia*, Linn. *Mant.* page 563, I have been favoured with by Doctor Klein, of *Tranquebar*, and the account of its economical uses by the Reverend Doctor John of the same place.

Description by Doctor Klein.

Bassia longifolia described.

Calyx, Perianth: monophyllum, 4-partitum; laciniis ovatis, acutis, coriaceis, extus tomento ferrugineo obductis, persistentibus.

Corolla monophylla, campanulata; tubo cylindraceo, inflato, carnoso, limbo 8-partito; laciniis lanceolatis, erectis.

Stamina, filamenta 16, brevissima, in duos ordines divisa, quorum octo ad incisuras laciniarum, octo in tubo corollæ inserta. Antheræ lineares, setaceæ, acutæ, extus pilosæ, limbo breviores.

Pistil: Germen superum, ovatum. Stylus setaceus, corolla duplo longior. Stigma simplex.

Pericarp: drupa oblonga, 1-3 sperma, carnosa, lactescens. Seminibus subtrigonis oblongis.

Arbor magna; ramis sparsis, erectis, horizontalibusque.

Folia sparsa, petiolata, lanceolata, acuta, integerrima, glabra, venosa.

Flores longe-pedunculati, axillares, solitarii, et aggregati.

1st. The

1st. The oil, pressed from the ripe fruit, is used as a common lamp oil, by those who cannot afford to buy the oil of the cocoa-nut. It is thicker, burns longer, but dimmer, smokes a little, and gives some disagreeable smell. Oil used in lamps,

2d. It is a principal ingredient in making the country soap, and therefore often bears the same price with the oil of the cocoa-nut. for making soap,

3. It is, to the common people, a substitute for ghee, and cocoa-nut oil, in their curries and other dishes. They make cakes of it, and many of the poor get their livelihood by selling these sweet oil cakes. in cookery,

4th. It is used to heal different eruptions, such as the itch, &c. and in medicine.

5th. The cake (or *sakey*) is used for washing the head; and is carried, as a petty article of trade, to those countries, where these trees are not found. The cake.

6th. The flowers, which fall in *May*, are gathered by the common people, dried in the sun, roasted, and eaten, as good food. They are also bruised, and boiled to a jelly, and made into small balls, which they sell or exchange, for fish, rice, and various sorts of small grain. Flowers eaten.

7th. The ripe fruit, as well as the unripe, is eaten by the poor, as other fruits. Of the unripe, the skin is taken off, and after throwing away the unripe kernel, boiled to a jelly, and eaten with salt and *capsicum*. Fruit eaten.

8th. The leaves are boiled with water, and given as a medicine, in several diseases, both to men, and to cattle. Leaves a medicine,

9th. The milk of the green fruit, and of the tender bark is also administered as a medicine. and milk,

10th. The bark is used as a remedy for the itch. and bark,

11th. The wood is as hard, and durable, as teak wood, but not so easily wrought, nor is it procurable of such a length for beams, and planks as the former; except in clay ground, where the tree grows to a considerable height; but, in such a soil, it produces fewer branches, and is less fruitful, than in a sandy, or mixed soil, which is the best suited for it. In a sandy soil, the branches shoot out nearer to the ground, and to a greater circumference, and yield more fruit. These trees require but little attention; beyond watering them during the first two or three years, in the dry season. Being of so great use, we have here Wood.

whole

whole groves of them, on high, and sandy grounds, where no other fruit trees will grow.

Flowers eaten by animals.

12th. We may add, that the owls, squirrels, lizards, dogs, and jackals, take a share of the flowers; but the vulgar belief is, that the latter, especially in the time of blossom, are apt to grow mad, by too much feeding on them.

Bassia obovata.

Bassia obovata, Forster's *Prod.* No. 200: a native of the Isle of *Tanna*, in the South Sea. Of this species I possess no other account than the definition, which corresponds with the habit of the genus. If Forster has left us no account of the uses of the tree, it may be worth while to make inquiry, when an opportunity offers.

Shea a species of the same genus.

Park's *shea*, or butter tree of *Africa*, we have reason, from his description, and figure, as well as from analogy, to suppose a species of this same genus. At page 352 of his travels in the interior of *Africa* he says, "The appearance of the fruit evidently places the *shea* tree in the natural order of *sapotæ*, (to which *bassia* belongs,) and it has some resemblance to the *madhuca* tree (*bassia latifolia,*) described by Lieutenant Charles Hamilton, in the *Asiatic* Researches, Volume I, page 300.

Park's account of it.

"The people were every where employed in collecting the fruit of the *shea* trees, from which they prepare a vegetable butter, mentioned in the former part of this work *. These trees grow in great abundance all over this part of *Bambarra*. They are not planted by the natives, but are found growing naturally in the woods; and in clearing woodland for cultivation, every tree is cut down but the *shea*. The tree itself very much resembles the *American* oak; and the fruit, from the kernel of which, first dried in the sun, the butter is prepared, by boiling the kernel in

* This commodity, *shea toulou*, which, literally translated, signifies *tree-butter*, is extracted, by means of boiling water, from the kernel of the nut, has the consistence and appearance of butter, and is in truth an admirable substitute for it. It forms an important article in the food of the natives, and serves also for every domestic purpose in which oil would otherwise be used. The demand for it is therefore great. Park's Travels in *Africa*. Page 26.

water

water, has somewhat the appearance of a *Spanish* olive. The kernel is enveloped in a sweet pulp, under a thin green rind; and the butter produced from it, besides the advantage of its keeping the whole year without salt, is whiter, firmer, and to my palate, of a richer flavour, than the best butter I ever tasted made of cows milk. The growth and preparation of this commodity seems to be amongst the first objects of *African* industry, in this and the neighbouring states; and it constitutes a main article of their inland commerce." Park's Travels in *Africa*, page 203-8.

In the following account of the *bassia butyracea*, by Mr. Gott, we find the people of *Almorah* eat the dregs, left after the finer parts have been extracted; consequently there can be little doubt of the wholesomeness of the pure *vegetable butter* itself. The thick oil of *bassia latifolia*, and *longifolia*, the natives of various parts of *India* either use alone, or mixed with ghee (clarified butter), in their diet. Bassia Butyracea.

On captain Hardwicke's departure for *England*, in the beginning of 1803, he gave me a small quantity of the above-mentioned substance, observing, that the only account he could give me of it was, that it was reported to him to be a vegetable product from *Almorah*, or its neighbourhood, where it is called *fulwah*, *or phulwarah*. In consequence of this information I applied to Mr. Gott, (who is stationed in the vicinity of that country,) to make the necessary enquiries; and from him I procured an abundance of well preserved specimens, at various times, in leaf, flower, and fruit. From these, and that gentleman's account of the tree, and its product, the foregoing description was taken. Some given the author in 1803.

The same sample, which I got from captain Hardwicke in *January*, 1803, I have still by me. It remains perfectly sweet, both in taste and smell. Its flavour is that of cloves; having, I presume, been perfumed with that spice, previously to its falling into his hands, a practice mentioned in the following narrative. At this instant the thermometer is at ninety-five, and for these six weeks, it has rarely been below ninety, and has often risen to one hundred, or more, yet it continues about as firm as butter is in *England* during winter. Keeps well. Consistent.

Mr. Gott's

Account of the tree.

Mr. Gott's account of the tree, and its product, is as follows: —

Native country.

The tree producing a fat-like substance, known in this country by the name of *phulwah*, is a native of the *Almorah* hills, and known there by the same name. The tree is scarce, grows on a strong soil, on the declivities of the southern aspects of the hills below *Almorah*, generally attaining the height, when full grown, of fifty feet, with a circumference of six. The bark, of such specimens as I have been able to obtain, is inclined to smoothness, and speckled; it flowers in *January*, and the seed is perfect about *August*, at which time the natives collect them, for the purpose of extracting the above substance. On opening the shell of the seed or nut, which is of a fine chesnut colour, smooth, and brittle, the kernel appears of the size and shape of a blanched almond; the kernels are bruised, on a smooth stone, to the consistency of cream, or of a fine pulpy matter; which is then put into a cloth bag, with a moderate weight laid on, and left to stand, till the oil, or *fat*, is expressed, which becomes immediately of the consistency of hog's-lard, and is of a delicate white colour. Its uses are in medicine; being highly esteemed in rheumatism, and contractions of the limbs. It is is also much esteemed, and used by the natives of rank, as an unction, for which purpose, it is generally mixed with an *utr* of some kind. Except the fruit, which is not much esteemed, no other part of the tree is used.

Nut.

Fat expressed.

Use.

Its difference from oil of mawa.

This tree is supposed to bear a strong affinity to the *mawa*, (*madhuca*, or *bassia latifolia;)* but the oil or *fat*, extracted from the seeds, differs very materially. The oil from the *mawa* is of a greenish yellow colour, and seldom congeals. That from the *phulwah* congeals immediately after expression, is perfectly colourless; and, in the hottest weather, if melted by art, will, on being left to cool, resume its former consistency. The oil from the seed of the *mawa*, if rubbed on woollen cloth, leaves as strong a stain as other oils or animal fat. The fatty substance from the *phulwah*, if pure, being rubbed on woollen cloth, will leave no trace behind.

The

The oil of *mawa* is expressed in considerable, quantities about *Cawnpoor*, and *Furruckabad*, and being mixed with, is sold as ghee.

This fatty substance very rarely comes pure from the hills, and receives more and more adulteration, (by adding the purest ghee,) as it passes down to the lower provinces: age gives it the firmness of pure tallow.

Additional Remarks by the same, in consequence of a few Queries transmitted to Mr. GOTT.

It is supposed there might be annually procured from twenty to thirty maunds, at the price of fourteen or fifteen rupees the maund. Farther remarks on it.

1st. It is never taken inwardly as a medicine, nor is it used in diet; further than that the dregs, after the purer fatty substance is expressed, are eaten, as a substitute for ghee, by the peasants, or labourers, who extract the fat.

2d. I have some pure, which has been by me ten months, and it has neither acquired colour, nor bad smell.

3d. After it is imported into *Rohilkhund*, it is scented with *utr*, (an essential oil,) and a little of the flour of *Indian* corn *(zea mays)* is added, to increase its consistency. N. B. This flour is added on account of its peculiar whiteness.

4th. If it is clean, and free from dirt, it never undergoes any purification; if the contrary, it is heated, and filtered through a coarse cloth.

5th. The flowers are never used. The pulp of the fruit is eaten by some; it is of a sweet, and flat taste.

The timber is white, soft, and porous; and is never made any use of by the natives. It is nearly as light as the *semul*, or cotton tree *(bombax heptaphyllum)*. Wood.

XI. *Observations*

XI.

Observations on Werner's Silex Schistosus Politorius, Polierschiefer, from Billin, in Bohemia *.

Where found.

THIS substance, called *polishing slate*, is found about three miles south of Billin, in Bohemia, immediately under the vegetable mould, and less than a yard deep. It is of a yellowish colour, and slaty texture; has an earthy appearance; and leaves a coloured mark on cloth. Between the fingers it is easily reduced to a powder, which is a little rough to the feel; it adheres strongly to the tongue; it is infusible. Its specific gravity according to Mr. Haberle is 0·6; and if left twelve hours in water 100 parts absorb 117. In Saxony it is known in the shops by the name of *silver tripoli*.

Described.

Stratum described.

In the place where I observed it, near the top of a pretty high hill, it forms the superior part of a stratum, which increases in density as you penetrate into it; and in some places at the depth of two yards it is compact, with a yellowish and somewhat shining aspect, like that of certain semiopals: but it is not so hard, or so heavy. From every thing I observed on the spot, the polishing slate is nothing more than a portion of this stratum, the texture of which is loosened and altered by decomposition. According to Mr. Reuss, who lives at Billin, the stratum includes remains of vegetables, and impressions of fish. Every thing besides indicates, that it is a recent alluvial production.

Mr. Bucholz has analysed both the polishing slate and the adhesive slate, *klebschiefer*, that accompanies the menilite of Menil-Montant, which had been considered as a variety of it: and as Mr. Klaproth has made a more full and complete analysis of the Klebschiefer than that he first gave the public, we shall here present the three analyses in a comparative view.

	Polishing slate by Bucholz.	Adhesive slate by Bucholz.	Adhesive slate by Klaproth.
Silex	79	58	62·5
Alumine	1	5	0·75
Lime	1	1·5	0·25
Oxide of Iron	4	9	4
Water	14	19	22
Magnesia		6·5	8
Carbon			0·75
	99	99	98·25

* Journal des Mines, N. 121, p. 77.

The oxide in the analysis of the adhesive slate by Bucholz was part of iron, part of manganese: and in the analysis by Klaproth the gas that escaped is included in the ·22 of water. He likewise found an alkali present, but in too small quantity to be weighed.

SCIENTIFIC NEWS, &c.

Tabellarische Uebersicht der chemisch einfachen und zusammengesetzten Stoffe: &c. A tabular View of simple and compound chemical Substances, with their Synonimes, according to the newest Discoveries: by Fred. Stromeyer, M. D. and Prof. at Gottingen. 32 whole Sheet Tables. 1806.

Stromeyer's chemical tables.

PROF. STROMEYER has here given a systematic arrangement of the different substances, that are the particular objects of chemical science, with a pretty copious collection of synonimes in German, Latin, French, and English. The only innovation he has allowed himself, according to his preface, is the classing of oil, sugar, starch, gluten, and several other vegetable and animal matters, as oxides with compound radicals, consisting either of carbon and hidrogen, or carbon, hidrogen, and nitrogen. Among these he makes wax differ from fixed oil only in being more oxided; and adipocere from fat in the same manner. By the by, the only name he gives for adipocere in the English column is *fat-wax*, a literal translation of *the German fettwachs*.

Investigation of the compounds of hidrogen with metals.

With these tables prof. S. sent me an account of a paper he read to the Gottingen Society, Oct. 12, 1805, containing part of the results of his chemical investigation of the union of hidrogen with metals. On the present occasion he confined himself to that of arsenic. This he observes succeeds best by digesting an alloy of fifteen parts of tin and one of arsenic with concentrated muriatic acid in a retort connected with the pneumatic apparatus. He was led to this by the observation of Proust, that muriatic acid completely frees tin from arsenic: and on this occasion he convinced himself by experiments, that the fetid hidrogen gas

Best process for arsenicated hidrogen.

gas evolved, when the tin of the shops is dissolved in muriatic acid, is not a compound of tin and hidrogen, as Fourcroy conjectures in his Chemical System, Vol. VI, p. 43, (English Ed.) but of arsenic and hidrogen. When arsenicated hidrogen gas is formed in the manner directed above, a very pure oximuriate of tin is obtained.

Mistake of Fourcroy.

Partly reduced to a liquid by cold.

Though the arsenicated hidrogen gas retains its aeriform state under every known degree of atmospheric temperature and pressure, prof. S. condensed it so far as to reduce it in part to a liquid, by immersing it in a mixture of snow and muriate of lime, in which several pounds of quicksilver had been frozen in the course of a few minutes.

Its properties.

The smell of this gas, he says, is not alliaceous, as has been said, though in the highest degree fetid and nauseating. Warm blooded animals, particularly birds, were killed in a few minutes in an atmosphere containing one tenth of this gas: but frogs and insects lived in it two or three hours. Blood fresh drawn from a vein became black after standing a few minutes in contact with it; and in six or eight hours a layer of reduced arsenic was visible on its surface. The rise of the fluid in the jar likewise proved, that absorption had taken place; but no such change appeared in blood exposed to pure hidrogen gas. Neither sirup of violets, infusion of litmus or turmeric, nor paper stained with them, had its colour in the least altered by the gas. Infusion of galls, and alkaline sulphurets or hidrosulphurets, have no observable action on it. It is not absorbed by alkalis; and scarcely in any perceptible degree by distilled water, particularly if freed from air as much as possible by long ebullition. If however the water contain atmospheric air, or if the arsenicated hidrogen gas be mixed with atmospheric air, not only absorption but decomposition takes place, part of the hidrogen and of the arsenic combining with oxigen so as to form water and brown oxide of arsenic, and part appearing in the form of pure hidrogen gas and metallic arsenic. Hence it is, that, as Proust observed, a jar in which this gas is kept over water will acquire a coating of arsenic and its oxide.

Effects on blood.

Action with reagents.

Absorbed by water only when containing air.

Effects of combustion with different pro-

The arsenicated hidrogen gas burns in contact with atmospheric air, and a thin coat of arsenious acid and brown oxide

oxide of arsenic is deposited on the sides of the vessel. If it be mixed with twice its volume of atmospheric air, the product of the combustion is arsenious acid and water. With six times its bulk of atmospheric air it will not take fire. A mixture of it with an equal part of atmospheric air cannot be fired by the electric spark. With an equal bulk of oxigen gas it detonates violently, and the products are water and arsenious acid: with only half, or a third, of oxigen gas, oxide of arsenic likewise is formed, and part of the metal is reduced. With five parts of oxigen gas it burns without detonation. Arsenic acid is formed in none of these processes. The combustion having been tried with various proportions of the two gasses in Volta's eudiometer, the mean of the experiments gave 0·72 of a cubic inch of oxigen gas as the proportion required to burn 1 inch of arsenicated hidrogen gas, in which the hidrogen is fully saturated with arsenic at the common temperature.

Portions of oxigen.

1 part requires 0·72 of oxigen to burn it.

Action of acids.

All acids, in which the oxigen is feebly combined, decompose arsenicated hidrogen gas. This phenomenon is very striking with nitric acid. While part of the hidrogen, being condensed by the oxigen of the acid, is converted into water, another part is set free. At the same time the whole [?] of the arsenic is separated in the metallic form, but is very quickly oxided by the nitric acid, and at length acidified. The nitric acid acquires a yellow colour, and bubbles of nitrous oxide gas are extricated from it. The gas that ultimately remains is pure hidrogen mixed with nitrous oxide. Prof. Stromeyer employs the action of nitric acid on the arsenicated hidrogen gas, to calculate the proportion of its principles, which, according to him are 10·600 arsenic, and 0·219 hidrogen.

Nitric.

Nitrous acid decomposes it instantaneously, and arsenious acid is deposited.

Nitrous.

Oxigenized muriatic acid decomposes it, part of the hidrogen and arsenic undergoing combustion, and the other being separated. Oxigenized muriatic acid gas brought into contact with it in narrow tubes acts upon it in the same manner as the liquid acid: but if the two gasses be mixed in a wide jar, the whole of the arsenic is instantly converted into arsenious acid, appearing as a white vapour;

Oxigenized muriatic.

pour; while part of the hidrogen forms water, and another part appears as pure hidrogen gas.

Other acids.

Sulphuric, phosphoric, and arsenic acid, equally decompose this gas; but the effect is produced very slowly, and the arsenic is deposited for the most part in the metallic form. In the decomposition of this gas by acids in general, a very perceptible increase of volume takes place at the commencement of the process.

Acid solutions of metals.

Most of the solutions of the metals in acids likewise decompose it. The hidrogen is in part burned by the disoxigenation of the metallic oxide, and in many cases by the disoxigenation of the acid likewise, with which the metal was combined, and forms water, while another part is converted into pure hidrogen gas. Thus the other component part, the arsenic, is separated, and in most cases, at least at the commencement, appears as a pure metal: but in general, if the acid have a weak affinity for oxigen and the oxide, or if the metal dissolved in it be highly oxided, the arsenic is soon converted into oxide, and thence into arsenious, or sometimes into arsenic acid. This is most striking with the corrosive muriate of mercury, which in this experiment is converted into mild muriate. This metallic salt is such a sensible test of arsenicated hidrogen gas, that it is capable of detecting it when mixed with ten thousand times its bulk of atmospheric air, or of pure hidrogen, as was found by experiment.

Corrosive muriate of mercury

a very sensible test of it.

Remarkable effect of turpentine.

Prof. Stromeyer concluded with a remarkable experiment, showing the effect of oil of turpentine on arsenicated hidrogen gas, all the phenomena of which however do not appear easily explicable. Ten cubic inches of the gas being confined over this essential oil, all the arsenic was separated in the course of ten hours, so as to leave the hidrogen gas pure. No perceptible deposition of metal or oxide took place; but the oil appeared milky and viscous; and after some time small sixsided crystals, terminating in pyramids, were found adhering to the sides of the vessel. These crystals, being set on fire, burnt like oil of turpentine, emitting at the same time a very distinguishable smell of arsenious acid. A similar appearance took place on transmitting arsenicated hidrogen gas through oil of turpentine.

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A

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AND

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VOL. XX.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers and Communications in the present Volume are, R. B.; Joseph Reade, M. D.; Dytiscus; Hemerobius; Mr Robert Banks; John Gough, Esq.; P.; A. B. C. D.; Lieutenant Henry Kater; Mr. J. Aston; Professor Vince; Mr. Daniel Dering Mathew; Dr. Clerke; Rev. J. Blanchard; Mr. T. Clifton; Mr. J. Wright; Mr. G. H. Willers; John Dickenson, Esq.; Mr. John Martin; Mr. Richard Drew; Mr. John Tatum; a Correspondent.

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The Engravings consist of 1. Mr. Middleton's Mode of Printing; 2. Relistian Tin Mine; 3. Singular Strata in a calcareous Mountain near Cressy; 4, 5. Ditto in the Department of Doubs; 6. Draining of the Pond of Citis; 7. Yenite, a new Mineral Substance; 8. Mr. Hardy's Compensation Balance; 9. Compensation Pendulum, by Mr. Henry Kater; 10. An irregular Production of the Cucumber; 11. Mr. Mathew's Scapement; 12. Planche's Apparatus for Succinic Acid; 13. Mr. Wright's artificial Horizon; 14. Mr. Daniel's Life Preserver from Shipwreck; 15. Lieutenant Bell's Method of saving Persons from stranded Ships; 16. Compound Sulphuret; 17. New Properties of Tangents; 18. Radiation and Reflection of Cold; 19. Mr. Richard Drew's Balance Level; 20. Mrs. D'Oyley's Method of rearing Poultry.

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JULY, 1808.

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AUGUST, 1808.

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MAY, 1808.

ARTICLE I.

An Attempt to ascertain the Time when the Potato (Solanum tuberosum) *was first introduced into the United Kingdom; with some Account of the Hill Wheat of India. By the Right Hon. Sir* JOSEPH BANKS, *Bart. K. B. P. R. S. &c.* *

THE notes on the introduction of the potato, which it is hoped will not be found uninteresting, were chiefly collected by my worthy and learned friend Mr. Dryander, some of them from authorities not easily accessible. Those on the wheat, though not within the immediate object of this Society, will, I hope, be considered as sufficiently interesting to be laid before them: could we trace the origin of any one of our cultivated plants, it may, and probably will, lead to the discovery of others. Notes collected chiefly by Mr. Dryander.

The potato now in use (solanum tuberosum) was brought to England by the colonists sent out by Sir Walter Raleigh, under the authority of his patent, granted by Queen Elizabeth, "for discovering and planting new countries, not possessed by christians," which passed the great seal in 1584. Potato introduced into England in 1586, by Sir W. Raleigh's colonists.

* From the Trans. of the Horticultural Society, Vol. I, Part I, p. 8.

Some of Sir Walter's ships sailed in the same year; others, on board one of which was Thomas Herriot, afterward known as a mathematician, in 1585; the whole however returned, and probably brought with them the potato, on the 27th July, 1586.

First account of them by Herriot.

This Mr. Thomas Herriot, who was probably sent out to examine the country, and report to his employers the nature and produce of its soil, wrote an account of it, which is printed in De Bry's collection of Voyages, Vol. I. In this account, under the article of roots, p. 17, he describes a plant called openawk: "These roots," says he, "are round, some as large as a walnut, others much larger: they grow in damp soil, many hanging together, as if fixed on ropes; they are good food, either boiled or roasted."

Gerard received the roots from Virginia.

Gerard, in his Herbal, published 1597, gives a figure of the potato, under the name of potato of Virginia; and tells us that he received the roots from Virginia, otherwise called Norembega.

First introduction into Ireland.

The manuscript minutes of the Royal Society, December 13, 1693, tell us, that Sir Robert Southwell, then president, informed the fellows, at a meeting, that his grandfather brought potatoes into Ireland, who first had them from Sir Walter Raleigh.

Considered as a delicacy in England in 1597.

This evidence proves, not unsatisfactorily, that the potato was first brought into England, either in the year 1586, or very soon after, and sent thence to Ireland, without delay, by Sir Robert Southwell's ancestor, where it was cherished and cultivated for food before the good people of England knew its value; for Gerard, who had this plant in his garden in 1597, recommends the roots to be eaten as a delicate dish, not as common food.

Conveyed earlier from America to Spain, and thence to Italy.

It appears, however, that it first came into Europe, at an earlier period, and by a different channel; for Clusius, who at that time resided at Vienna, first received the potato in 1598, from the governor of Mons, in Hainault, who had procured it the year before from one of the attendants of the pope's legate, under the name of taratoufli; and learned from him, that in Italy, where it was then in use, no one certainly knew whether it originally came from Spain, or from America.

Peter Cieca, in his Chronicle, printed in 1553, tells us, chap. xl, p. 49, that the inhabitants of Quito, and its vicinity, have, beside mays, a tuberous root, which they eat, and call papas. This Clusius guesses to be the plant he received from Flanders; and this conjecture has been confirmed by the accounts of travellers, who have since that period visited the country. Mentioned in 1553.

From these details we may fairly infer, that potatoes were first brought into Europe from the mountainous parts of South America, in the neighbourhood of Quito; and, as the Spaniards were the sole possessors of that country, there is little doubt of their having been first carried into Spain, but as it would take some time to introduce them into use in that country, and afterward to make the Italians so well acquainted with them as to give them a name*, there is every reason to believe they had been several years in Europe, before they were sent to Clusius. General inference.

The name of the root, in South America, is papas, and in Virginia, it was called openawk; the name of potato was therefore evidently applied to it on account of its similarity in appearance to the battata, or sweet potato; and our potato appears to have been distinguished from that root, by the appellative of potato of Virginia, till the year 1640, if not longer†. Etymology of the name.

Some authors have asserted, that potatoes were first discovered by Sir Francis Drake, in the South Seas; and others, that they were introduced into England, by Sir John Hawkins; but in both instances the plant alluded to is clearly the sweet potato, which was used in England as a delicacy, long before the introduction of our potatoes; it was imported in considerable quantities from Spain, and the Canaries, and was supposed to possess the power of restoring decayed vigour. The kissing comfits of Falstaff‡, and other confections of similar imaginary qualities, with which our an- The sweet potato introduced into England earlier. Its reported properties.

* Taratoufli signifies also truffles.

† Gerard's Herbal, by Johnson, p. 729.

‡ "Let it rain potatoes, and hail kissing comfits." Merry Wives of Windsor, Act v, Scene 5.

Parkinson's Paradisus Terrestris, p. 518. Gerard's Herbal, 1697, p. 780.

cestors were duped, were principally made of these, and of eringo roots.

The potatoes themselves were sold by itinerant dealers, chiefly in the neighbourhood of the Royal Exchange, and purchased when scarce at no inconsiderable cost, by those who had faith in their alleged properties. The allusions to this opinion are very frequent in the plays of that age.

Every anecdote that tends to throw light on the introduction, or on the probable origin, of plants now cultivated for use, is certainly interesting, even though it is not quite perfect; I venture, therefore, to add the following.

Small seeds called hill wheat.

Seven or eight years ago, Mr. Lambert brought to me a small paper of seeds, on which was written, "*Hill Wheat*;" I opened it, and found the seeds contained to be scarce larger than those of our wild grasses; but when viewed through a lens, they perfectly resembled grains of wheat.

Produced spring wheat of the ordinary size.

Of these seeds, he was so good as to spare me a few, which I sowed in a garden, the remainder he sowed; our crops very unexpectedly proved to be wheat of the spring kind, and the usual size, the grains of which were nearly, if not quite, as large as those of the ordinary spring wheat.

Came from some part of India.

On this, Mr. Lambert applied to Mrs. Barrington, from whom he had received the seeds, for information of the country from which they came; but she had, among the multiplicity of seeds received by her about the same time, forgot the exact history of them; all she knew was, that they came from India, but from what part of India, she did not recollect.

From the writing on the paper, "*Hill Wheat*," it is probable they came either from the Peninsula, or from the hilly country, far within land from Bengal, as the province of Bengal itself is a flat alluvial soil, entirely level.

Highly desirable to learn its origin, and whether wild.

The hill wheat, however, is no doubt known to some persons, who either are now in India, or have returned from it into this country; and it is certainly a matter of some importance to know, what they can inform us on the subject of it; especially whether this wheat is a cultivated, or a wild plant; as we shall, if the latter is the case, ascertain two of the greatest desiderata of cultivators; the country where wheat

wheat grows spontaneously; and the nature of the grain in its original state, when unassisted by the fostering hand of man.

II.

Observations on the Structure of the Stomachs of different Animals, with a View to elucidate the Process of converting animal and vegetable Substances into Chyle. By EVERARD HOME, *Esq. F. R. S**.

THE observations on the stomachs of the porpoise†, and of ruminating animals, contained in two former communications, led me to believe, that the fourth cavity of the ruminant's stomach, while the animal is alive, is always divided, in a greater or less degree, into two portions, in one of which is included the plicated structure, in the other, the villous. In some genera, this division is permanent, as in the camel and that tribe; in others only occasional, as in the bullock, deer, sheep, &c.

Fourth cavity of the ruminant's stomach divided into two portions.

If this opinion should be found to be true with respect to animals in general, it will throw considerable light on the processes carried on in the stomach, and lead us to conclude, that the food undergoes two changes in it, the one preparatory to the other, and that it is the last of these, which forms the chyle.

The food therefore undergoes two changes in it.

With a view to investigate still farther this very interesting subject, I have been led to examine the internal structure of the stomachs of different animals.

In this inquiry it will be found, that the same substances are digested by stomachs varying considerably from each other, and many of these varieties can at present in no other way be accounted for, than by referring them to the general principle, which pervades the structure of animals, making them run into one another by a regular series of minute

A regular gradation of form in animals.

* Abridged from the Philos. Trans. for 1807, Part II, p 129.

† See our last vol.

changes

changes of form, so as to compose one connected chain, from which we derive the fullest evidence of the power and wisdom of their Creator.

Three different structures in the stomach of ruminants,

The stomachs of all ruminating animals have three different structures; the first of these is cuticular; the second has a secreting surface, thrown into folds, on which are seen the orifices of glands; and the third is smooth and more delicate in its texture.

and also of non-ruminants.

In the following account, it will be found that three similar structures are met with in the stomachs of quadrupeds which do not ruminate, and that the gradation between the most complex and most simple stomachs forms a uniformly connected series, of greater extent than has been hitherto supposed.

To complete the view of this subject is too extensive a pursuit for an individual, whose professional duties occupy so large a portion of his time as mine necessarily do. All that can be expected from one so circumstanced is to give a general outline, leaving the minuter parts to be filled up by those who have more leisure, but by no means more zeal, for studies of this kind.

Best mode of examining the stomach, to ascertain its shape and structure.

As the object of the present inquiry is to determine with as much accuracy as possible the shape the stomach puts on, while performing its functions in the living body, and the structure, which belongs to the different parts of its internal membrane, it became necessary to consider what would be the best mode of making such examinations. It was found, that the stomach ought not to be in a distended state at the time of the animal's death, for when this is the case, the air which is let loose, or even the shaking of the contents, elongates or stretches the muscular fibres, so as to enlarge the cavity, and give it a form, by no means natural to it. This partly arises from the weakness of the muscular fibres themselves; but principally from the effect of death upon this organ, which destroys the rigidity of its muscular fibres, so that they become easily elongated, even when much shortened at the time death takes place. It is necessary to mention this circumstance, as it is the reverse of what happens in the voluntary muscles, which are generally known to become rigid at that time, and it accounts for the real form of the

Death destroys the rigidity of its fibres, the reverse of which takes place in voluntary muscles.

the stomach having been much less frequently noticed than was naturally to be expected.

To come at the real form of the stomach, it must be seen recently after death, before its muscles have been disturbed; in this state a gentle and gradual distension with air shows both the permanent divisions of its cavity, if there be any, in the best possible manner, and also any occasional muscular contractions, that are employed during life. Should be examined soon after death.

The internal membrane is only to be met with in a natural state recently after death, since the secretion from the solvent glands frequently acts upon it, and destroys the surface, and the slightest degree of putrefaction, which comes on very quickly in this cavity, prevents the nicer distinctions of structure from being detected. Its internal surface soon acted upon after death.

To make an accurate examination of the different parts of this membrane, it is necessary, that its folds should be extended, and the mucus commonly found adhering to it removed; which is most readily effected, and with the least disturbance, by inverting the stomach and gradually distending it; and in this state only can the relative situation of the different structures be ascertained with exactness. Best method of examining the internal membrane.

In examining stomachs, with the attention directed to all the circumstances above mentioned, it is found, that, in a recent state, the internal membrane is often completely obscured by mucus, which in many instances is inspissated, and puts on the appearance of a cuticular covering, from which it is with difficulty distinguished; in others it resembles a fine villous surface, so very tenacious is its nature; and where the membrane is irregular it adheres with unusual firmness. Often obscured by mucus.

The internal membrane of most stomachs is found to be considerably more extensive than any of the other coats, and much more so than it appears to be on a superficial examination; for it is not only thrown into longitudinal and transverse folds, but is subdivided by slight fissures into a number of small portions varying in shape and size in different parts of the same stomach, but generally smallest near the pylorus. This appearance was at first mistaken for the real internal structure of the membrane; but when inverted and distended, so as to be put upon the stretch, all Much more extensive in general than the other coats.

these

these disappeared, and it became very thin and smooth. This is seen most readily in the human stomach, and in those of carnivorous animals.

Cannot perform its functions when overdistended.

Such distention enables us to examine the internal structure of parts, but this is not to lead us away from their more natural appearance; since the functions of this membrane could no more go on were it unfolded to a great extent, than the muscular actions of the outer coat, in an overstretched state of its fibres.

Hence a child killed by eating too much.

In proof of this observation, I have known an instance of a child three years old, who, being left alone at dinner, ate so large a quantity of apple-pudding, that it died, which raised suspicion of its having been poisoned. On examination after death, the whole stomach was distended to its utmost extent, and rendered quite tense, which was the only apparent cause of the child's death.

Mr. Home next proceeds to describe the stomachs of a considerable number of animals, his able and minute examination of which is illustrated by several excellently engraved plates; after which he gives the following general observations.

Process of digestion most complex in ruminating animals.

First stage of this process.

In the stomachs of ruminating animals, the processes the food undergoes before it is converted into chyle are more complex than in any others. It is cropped from the ground by the fore teeth, then passes into the paunch, where it is mixed with the food in that cavity; and it is deserving of remark, that a certain portion is always retained there; for although a bullock is frequently kept without food seven days before it is killed, the paunch is always found more than half full; and as the motion in that cavity is known to be rotatory by the air balls found there being all spherical or oval with the hairs laid in the same direction, the contents must be intimately mixed together; the food is also acted on by the secretions belonging to the first and second cavities; for although they are lined with a cuticle, they have secretions peculiar to them. In the second cavity these appear to be conveyed through the papillæ, which in the deer are conical; and when examined by a lens the focus of which is $\frac{1}{2}$ an inch, they are found to have three distinct orifices, and that part of each papilla next the point is semitransparent.

These

These secretions are ascertained by Dr. Stevens's experiments to have a solvent power in a slight degree, since vegetable substances contained in tubes were dissolved in the paunch of a sheep*.

The food thus mixed is returned into the mouth, where it is masticated by the grinding teeth; it is then conveyed into the third cavity, in which it would appear from the gas† let loose, that a decomposition takes place, and thence it is received into the upper portion of the fourth cavity. Second stage.

The changes which are produced on the food in the first three cavities are only such as are preparatory to digestion, and it is in the fourth alone this process is carried on. In the plicated portion the food is acted on by the secretion of the solvent glands; and in this portion of the cavity of the deer's stomach small orifices are seen in the internal membrane leading to cavities, the size of a pin's head, which I consider to be the openings of these glands, since they bear some resemblance to those of other stomachs. In the lower portion the formation of chyle is completed. The 4th stomach the true seat of digestion. Formation of the chyle completed in in its lower portion.

In birds with gizzards the food goes through very similar changes; it is picked up by the bill, which in smaller birds separates the husk from the seed, it then passes into the crop, where it is acted on by the secretions of that cavity, after which it is received into the gizzard, to undergo the same change produced by the grinding teeth of the ruminants; the secretion of the solvent glands is then poured upon it, acting upon the nutritious part before it is spread upon the glandular structure at the orifice of the gizzard, in which last situation it is formed into chyle. Birds with gizzards.

In the whale tribe, the first cavity, although lined with a cuticle, has secretions peculiar to it, and therefore corresponds with the first and second of the ruminants, and with the crops of birds with gizzards: it answers however a Whale tribe.

* *Dissertatio Physiologica inauguralis de Alimentorum concoctione, Autore* Edwardo Stevens, Edinb. 1777.

† Mr Davy and Mr. W. Brande examined this gas, and found it to be inflammable, and not to contain carbonic acid; which establishes a difference between this process and fermentation. Not the fermentative process.

farther

farther purpose, by dissolving its contents sufficiently to prevent the necessity of rumination, or the use of a gizzard. The second cavity performs the same office as the plicated portion of the fourth cavity of the ruminant, and the fourth is that in which the chyle is formed. This complex structure of the stomach in the whale tribe, although it gives it an appearance of great similarity to that of the ruminant, is not at all formed on the same principle, since the additional cavities in the ruminant are to prepare the food for the process of digestion; while in the whale tribe no such preparation is required; but as the fishes they feed upon are swallowed whole, and have large sharp bones which would injure any surface not defended by cuticle, a reservoir became necessary, in which they may be dissolved and converted into nourishment, without retarding the digestion of the soft parts. The very narrow communication between the second, third, and fourth cavities, resembles the opening between the cardiac and pyloric portion in fishes.

Different from the ruminating tribe, though a similarity in structure.

The stomachs of this tribe of animals are therefore introduced here, as being next in order with respect to the complexity of parts, and having by the division of them led me to the present investigation, although it is by no means their proper place, with respect to their mode of digestion.

Animals nearest to the ruminants.

The animals, nearest allied to the ruminants in their mode of digestion, are those which, like them, retain a portion of food in the cardiac extremity of the stomach, that it may undergo a change, before it is submitted to the action of the solvent liquor; and when so hard as to render it necessary, return it again into the mouth, to be masticated a second time.

Hare and rabbit.

Ruminate occasionally.

The hare and rabbit are of this kind; the cardiac portion of the stomach is never completely emptied, and they occasionally ruminate. In proof of both these facts, a rabbit, which had been seven days without food, died, and the cardiac portion of the stomach was found to contain more than half of its usual quantity of contents: they were rather softer than common, and a number, amounting to 50 or 60 of distinctly formed pellets, the size of shot, were collected together in the cardiac extremity, immediately

below

below the œsophagus. These could not have been formed at the time of eating, since in seven days the action of the stomach would have destroyed their shape. They must therefore have acquired it by the animal chewing the cud.

This second class of ruminants have no cuticular lining to their stomachs, which may arise from their being more cautious feeders than the others, so that they are not liable to receive into the stomach any thing which can injure its internal membrane. All that portion of the stomach, which corresponds with the first cavity in the true ruminant, has one uniform structure, and is covered with a viscid mucus, but beyond this there are orifices, which I believe belong to solvent glands of a very small size; and toward the pylorus, the glandular appearance is of a different kind; so that in these stomachs the changes the food goes through correspond very closely with those it undergoes in ruminants. Their difference from the true ruminant.

The next order of animals with respect to digestion consists of the beaver and dormouse. These, both in the shape and general appearance of the stomach, as well as of the teeth, bear a close affinity to the hare; but they have a glandular structure peculiar to them, which seems to correspond with the solvent glands of other animals; and as the dormouse empties its stomach completely, there is reason to believe, that the beaver does so likewise, and that neither of them ruminates, since the regurgitation of the food would be attended with difficulty from the situation of these glandular structures; and it is probable, as they do not ruminate, the increased secretion of a solvent liquor renders it unnecessary. Beaver and dormouse. Probably do not ruminate.

The changes the food undergoes in these stomachs are only two; it is acted upon by the secretion from the solvent glands, and afterward converted into chyle by the secretion of those near the pylorus. This is a less complex process than in many of the stomachs not yet taken notice of, and is exactly similar to what takes place in carnivorous animals; it may therefore be considered as a connecting link between the ruminating and carnivorous stomachs. Link between the ruminating and carnivorous.

After these, which form a regular series from the ruminants, are the stomachs with cuticular reservoirs, in which the Water rat.

the food is macerated, before it is submitted to the process of digestion. Animals of this kind are the water rat, in which there is a permanent division between the cuticular cavity and the digestive part of the stomach; the common rat and the mouse, in which there is only a muscular one. The cuticular lining is thick and impervious; beyond it is a glandular part, that secretes a mucus found adhering to its surface; and farther on are orifices, which appear to belong to the solvent glands. These animals do not ruminate, and there is a kind of provision in nature to prevent regurgitation of the food. When kept without food for several days they completely empty their stomachs,

Common rat and mouse.

Horse and ass.

The horse and the ass, although animals in all other respects different, correspond so very closely in the structure of their stomachs with the rat and mouse, that their stomachs must be considered of the same kind.

In these the food is rendered easy of solution by remaining in the cuticular reservoirs; it is then acted on by the solvent liquor, and in the pyloric portion converted into chyle.

Kanguroo.

The stomach of the kanguroo, from the peculiarities of its structure, forms an intermediate link between the stomachs of animals which occasionally ruminate, those which have a cuticular reservoir, and a third kind not yet noticed, with processes or pouches at their cardiac extremity, the internal membrane of which is more or less glandular. The kanguroo is found to ruminate, when fed on hard food. This was observed by Sir Joseph Banks, who had several of these animals in his possession, and frequently amused himself in observing their habits. It is not however their constant practice, since those kept in Exeter Change have not been detected in that act. This occasional rumination connects the kanguroo with the ruminant. The stomach having a portion of its surface covered by cuticle, renders it similar to those with cuticular reservoirs; and the small process from the cardia gives it the third distinctive character; indeed it is so small, that it would appear placed there for no other purpose.

Occasionally ruminates.

Stomach occasionally divided into a

The kanguroo's stomach is occasionally divided into a greater number of portions than any other, since every part of

of it, like a portion of intestine, can be contracted separately; and when its length, and the thinness of its coats are considered, this action becomes necessary to propel the food from one extremity to the other. Such a structure of stomach makes regurgitation of its contents into the mouth very easily performed. The food in this stomach goes through several preparatory processes; it is macerated in the cuticular portion; it has the secretion from the pouch at the cardia mixed with it; and is occasionally ruminated. Thus prepared, it is acted on by the secretion of the solvent glands, which probably are those met with in clusters in the course of the longitudinal bands, and afterward converted by the secretions near the pylorus into chyle.

greater number of portions.

The animals, whose stomachs have processes or pouches at their cardiac extremity, are the kanguroo, hog, pecari, hippopotamus, and elephant.

Animals with processes at the cardia.

The pecari's stomach bears the nearest resemblance to those with cuticular reservoirs, having a portion of its surface lined with cuticle; but it only extends to a small distance from the termination of the œsophagus, and is not continued over any part of the great curvature.

Pecari.

The hippopotamus's stomach I have never seen, and Daubenton's description and engravings are taken from that of a fœtus; so that the structure of its minute parts is imperfectly known; but there is no doubt of there being a large pouch on each side of the cardiac portion, and there is reason to believe, that no part of the cavity of the stomach is lined with cuticle.

Hippopotamus

The elephant's stomach is the most simple of this kind. It has no cuticular lining; the elongation at the cardia is only a continuation of the general cavity, distinguished from it by the membranous septa; and the broad one may act as a valve, and occasionally preclude the food from passing.

Elephant.

In these stomachs the pouches at the cardia can only be connected with the preparation of the food, softening it by means of their secretions, or retaining it within their cavities; the other glandular structures are similar to those in the ass and rat, only more conspicuous.

It is deserving of remark, that the internal structure of the stomachs fitted for digesting vegetable substances, corresponds

In phytivorous animals the structure of

The stomach less analogous to that of the teeth than commonly supposed.

responds much less with the kind of teeth, than it has been generally supposed to do. The animals with chissel teeth have no uniformity in the structure of their stomachs; those of the beaver and dormouse being of one kind; the hare's and rabbit's of another; the squirrel's of a third, resembling that of the monkey; the guinea pig's of a fourth, differing from that of the squirrel, in there being a greater disproportion between the thickness of the coats of the cardiac and pyloric portions; the rat tribe of a fifth, which resembles the stomach of the horse and ass, animals whose teeth have a very different form.

Greater analogy between the stomach and weapons of defence.

On the other hand, all the ruminants with horns have one structure of stomach; all those with fighting teeth another, as has been observed in a former paper; also all the animals with projecting tusks have the pouches at the cardia, which appear to be peculiar to them, although there is no connection we yet know of between these weapons of defence and the stomach.

Elephant.

As the elephant's grinding teeth are the best fitted for preparing vegetable food for digestion, so the stomach in its structure approaches nearer to those of carnivorous animals.

The stomachs of which the structure has been hitherto considered belong to animals that feed on vegetables, and chiefly on the leaves, roots, and branches of plants. In the gradation towards carnivorous stomachs, we are next to take notice of those that belong to animals whose principal food is the fruits of trees, which appear to require less preparation for the process of digestion; of this kind are the stomachs of the squirrel and monkey. These in their general appearance resemble very closely the human stomach; at least the few opportunities, which have occurred to me of examining them, have not enabled me to detect any circumstances in which they differ.

Animals that feed on fruits.

The human stomach appears to be the uniting link between those that are fitted only to divest vegetable substances, and those that are entirely carnivorous; and yet we find in its internal structure it is in every material respect similar to those of the monkey and squirrel, which only digest vegetable productions, and also equally similar to those of

Human stomach.

of carnivorous animals. From this it would appear, that many parts of vegetables are as easily digested as animal substances, and require the same organs for that purpose; but others again require a particular preparation, without which they cannot be converted into chyle; of these last the principal are the grasses, which the human stomach is unable to digest.

Grasses not digestible by it.

The human stomach is divided into a cardiac and pyloric portion, by a muscular contraction similar to those of other animals; and as this circumstance has not before been taken notice of, it may be necessary to be more particular in describing it.

Divided into two portions.

The first instance, in which this muscular contraction was observed in the human stomach, was in a woman, who died in consequence of being burnt. She had been unable to take much nourishment for several days previous to her death. The stomach was found empty, and was taken out of the body at a very early period after death. It was carefully inverted to expose its internal surface, and gently distended with air. The appearance it put on has been already described. The contraction was so permanent, that after the stomach had been kept in water for several days in an inverted state, and at different times distended with air, the appearance was not altogether destroyed.

First instance of it observed.

Since that time I have taken every opportunity of examining the human stomach recently after death, and find that this contraction in a greater or less degree is very generally met with. The appearance which it puts on varies: sometimes it resembles that of the ass, so that this effect is not produced by a particular band of muscular fibres, but arises from the muscular coat in the middle portion of the stomach being thrown into action: and this for a greater or less extent, according to circumstances. When this part of the stomach is examined by dissection, the muscular fibres are not to be distinguished from the rest.

General: but varies in appearance.

If the body be examined so late as 24 hours after death, this appearance is rarely met with, which accounts for its not having before been particularly noticed.

Seldom observable 24 hours after death.

Perrault found a contraction somewhat similar in a lion's stomach, which appeared to him extraordinary, as it was only

Lion's stomach similar.

only met with in one instance out of four, that were examined. He gives a drawing of the appearance, but makes no comments on the cause of the contraction*.

Attempt to produce it in a cat.

Finding this contraction was met with, when the human stomach was nearly empty, I endeavoured to produce it in the cat, by having the stomach emptied by means of an emetic a short time before the animal's death. This did not however succeed; for although in the contracted state the line between the cardiac and pyloric portions was very distinct, and the last more contracted than the former, yet upon distending the stomach with air, the middle portion relaxed equally with the rest. The contraction at this part is therefore only to be seen, when these fibres have acted independently of the others; which takes place while the functions of the stomach are going on, but cannot be artificially produced.

Cannot be produced artificially.

Dog.

In examining the stomach of a dog in a contracted state, and afterward when it was distended, the line between the two portions could be distinctly perceived, even after the contraction was destroyed, by the longitudinal folds of the internal membrane of the pyloric portion all terminating there.

Food dissolved in the cardiac portion;

That the food is dissolved in the cardiac portion of the human stomach, is proved by this part only being found digested after death; the instances of which are sufficiently numerous, to require no addition being made to them. This could not take place unless the solvent liquor was deposited there. Mr. Hunter goes so far as to say, in his paper on this subject, "there are few dead bodies in which the stomach at its great end is not in some degree digested."

* La conformation du ventricule étoit particulière, et bien différent en ce sujet de celle, que nous avons trouvés aux autres lions, que nous avons dissequés, où le ventricule étoit semblable à celui des chiens et des chats; ayant un fond ample et large vers l'orifice supérieur qui alloit toujours en s'étrecissant vers le pylore; mais celui ci avoit le fond separé en deux, en quelque façon comme les animaux qui ruminent. Ce forme particulière du ventricule n'étoit qu'en un seûl des quatre animaux de cette espèce que nous avous dissequés, sçavoir deux lions et deux lionnes.

Mémoires pour servir à l'Histoire Naturelle des Animaux, dressée par M. Perrault, Fol. Ed. 1676.

That

That the chyle is not formed there, and also that it is completely formed before the food passes through the pylorus, is proved by the result of some experiments of Mr. Hunter's, made upon dogs in the year 1760; and as they were instituted for a very different purpose,—that of determining whether the gastric juice is acid or alkaline,—the results were detailed without any possible bias.

but the chyle formed in the pyloric.

The stomach of seven dogs were examined immediately after death, which took place while digestion was going on; and among other observations the following appear among Mr. Hunter's notes made at the time:

Dogs examined by Mr. Hunter.

" In all the dogs the food was least dissolved, or even " mixed, towards the great end of the stomach, but became " more and more so towards the pylorus; and just within the " pylorus it was mixed with a whitish fluid like cream, which " was also found in the duodenum."

He afterward adds; " It is plain, that digestion is com- " pleted in the stomach, as none of the crude food is found " beyond that cavity; and even within the pylorus there is the " same white fluid, that is met with in the duodenum."

From the result of these experiments, as well as from the analogy of other animals, it is reasonable to believe, that the glands situate at the termination of the cuticular lining of the œsophagus, which have been described, secrete the solvent liquor, which is occasionally poured on the food, so as to be intimately mixed with it before it is removed from the cardiac portion: and the muscular contraction retains it there, till this takes place.

Glands that secrete the solvent liquor.

Such contraction being occasionally required in the stomach, accounts for its being more or less bent upon itself, which renders it more readily divided into two portions by the action of the muscular fibres at that part where the angle is formed.

Curvature of the stomach accounted for.

It accounts for men occasionally ruminating, a process, which, without such a contraction, could hardly take place. That some men ruminate, the accounts of authors are sufficiently explicit to put beyond all doubt; particularly the instances collected by Peyer from Fabricius *ab Aquapendente* and others, as well as from his contemporaries, in all six or seven

Men occasionally ruminate.

instances. Of these, two were examined after death. In one of them the œsophagus was unusually muscular, but nothing particular was met with in the stomach: in the other, nothing is said of the œsophagus, but the internal surface of the stomach was very rough.

The fact, however, does not rest on these authorities, since a case of this kind has come within my own observation.

An instance observed by the author.

The instance to which I allude, is a man 19 years of age, blind, and an ideot from his birth, who is now alive. He is very ravenous, and they are obliged to restrict him in the quantity of his food, since, if he eats too much, it disorders his bowels. Fluid food does not remain on his stomach, but comes up again. He swallows his dinner, which consists of a pound and a half of meat and vegetables, in two minutes, and in about a quarter of an hour he begins to chew the cud. I was once present on this occasion. The morsel is brought up from the stomach with apparently a very slight effort, and the muscles of the throat are seen in action when it comes into the mouth; he chews it three or four times, and swallows it; there is then a pause, and another morsel is brought up. This process is continued for half an hour, and he appears to be more quiet at that time than at any other. Whether the regurgitation of the food is voluntary or involuntary cannot be ascertained, the man being too deficient in understanding, to give any information on the subject.

The contents not discharged by the first effect of an emetic,

This contraction of the stomach also explains the circumstance of its contents not being completely discharged, by the first effect of an emetic, which only empties the cardiac portion: the contraction preventing the pyloric portion from being emptied till the violence of the straining ceases, at which time relaxation takes place.

Cramp of the stomach.

It may also enable us to account for many symptoms that occur in the diseases of this organ, particularly the violent cramps, to which it is liable: as from the situation of the pain they probably arise from preternatural contractions of these muscular fibres.

Indigestion.

On the other hand, the indigestion met with in debilitated stomachs may proceed from this part having lost its proper degree of action, and therefore the food is not

not retained in it so as to be acted on by the different secretions.

This however is not the place to enter into these subjects; the object of the present investigation has been to collect facts in comparative anatomy, that may throw light upon the conversion of the food into chyle, and to abstain as much as possible from all matters of opinion;—no easy forbearance in going over ground, that has given rise to so many theories, and which the mind cannot contemplate, without forming a variety of conjectures.

Truly carnivorous stomachs resemble the human.

The stomach of the truly carnivorous quadruped appears to be made up of the same parts as the human. In the lynx, the different structures are more strongly marked, the solvent glands are more conspicuous, the pyloric portion is more bent, which renders the division between it and the cardiac more distinct, the muscular coats of the pyloric portion are much stronger, and on its internal surface glands are very obvious, which are not to be observed in the human.

The lynx.

Peculiarities in some.

The stomachs of some carnivorous animals have glandular structures peculiar to them; these are in the pyloric portion; there are also similar glands in the stomachs of some graminivorous animals, as has been already explained. The following may be mentioned as instances of this kind.

Lynx.

In the lynx, a glandular zone surrounds the orifice of the pylorus.

Mole.

In the mole, there is a similar zone.

Stoat and armadillo.

In the stoat, and armadillo, there is a glandular structure near the pylorus.

Sea otter.

In the sea otter, there is a glandular structure extending from the pyloric portion into the duodenum, described in a former paper.

Gradation from carnivorous beasts to birds of prey.

In tracing the gradation from carnivorous quadrupeds to birds of prey, it would have been natural to expect, that the bat, which has wings, and lives on animal food, should form an intermediate link: this, however, is not the case; the stomach of the long-eared bat resembles those of small carnivorous quadrupeds; that of the vampyre bat, which will be found to live on vegetables, has more the appearance of an intestine, and may, from its form, be mistaken for the

Long-eared bat.

Vampyre.

 cæcum

cæcum and colon; in this respect it approaches the kanguroo, and still more closely, the kanguroo rat; its cardiac portion is shorter, and its pyloric longer, than in the stomach of that animal, and there is no valvular structure at the orifice of the cardia.

Ornithorinchus, the only real link between beasts and birds.

The only real link between the stomachs of quadrupeds and birds is that of the ornithorinchus, which, however, is more an approach to the gizzard, being lined with a cuticle, containing sand, and having the same relative situation to the œsophagus and duodenum. The food of this animal is not known; it is probably of both kinds; the papillæ at the pylorus, which appear to be the excretory ducts of glands, are peculiar to it.

Birds of prey.

The stomachs of birds of prey are formed upon the same principle as those of carnivorous quadrupeds, but their cavity is more a continuation of the œsophagus, and the solvent glands are more conspicuous and numerous. Both these differences may be accounted for from their swallowing their prey whole, or nearly so; which requires a more direct passage into the stomach, and a greater quantity of secretion from the solvent glands, than when the food has undergone mastication. The cardiac portion of these stomachs is very distinct from the pyloric.

Snakes, turtles, and fishes.

In snakes, turtles, and fishes, the stomachs have the same characters as in birds of prey, but the cardiac and pyloric portions are still more distinct from each other, and the solvent glands are in general distributed over a larger surface of the cardiac portion.

General conclusions.

From the series of facts and observations which have been adduced, the following conclusions may be drawn.

That the solvent liquor is secreted from glands of a somewhat similar structure in all animals, but much larger and more conspicuous in some than others.

That these glands are always situate near the orifice of the cavity, the contents of which are exposed to their secretion.

That the viscid substance, found on the internal membrane of all the stomachs that were examined recently after death, is reduced to this state by a secretion from the whole surface of the stomach, which coagulates albumen. This appears to be

be proved, by every part of the fourth cavity of the calf's stomach having the property of coagulating milk.

This property in the general secretion of the stomach leads to an opinion, that the coagulation of fluid substances is necessary for their being acted on by the solvent liquor; and a practical observation of the late Mr. Hunter, that weak stomachs can digest only solid food, is in confirmation of it. Weak stomachs can digest only solid food.

That in converting animal and vegetable substances into chyle, the food is first intimately mixed with the general secretion of the stomach, and after it has been acted on by them, the solvent liquor is poured upon it, by which the nutritious part is dissolved. This solution is afterward conveyed into the pyloric portion, where it is mixed with the secretions peculiar to that cavity, and converted into chyle.

The great strength of the muscles of the pyloric portion of some stomachs will, by their action, compress the contents, and separate the chyle from the indigestible part of the food.

In animals whose food is easy of digestion, the stomach consists of a cardiac and pyloric portion only; but in those whose food is difficult of digestion, other parts are superadded, in which it undergoes a preparation before it is submitted to that process.

III.

Description of a Machine for Printing Paper Hangings. By Mr. John Middleton, *of St. Martin's-Lane.**

BY this machine the printer works with greater facility and dispatch than in the usual way; and the tereboy, who could with great difficulty serve one sieve, can by its means serve two with ease to himself. For this improvement the honorary silver medal was voted to Mr. Middleton by the Society of Arts. The following description shows the nature of this apparatus for facilitating the operations in paper-staining, and the mode of using it both for light and dark grounds. Advantages of this machine.

* From the Transactions of the Society of Arts, for 1807, p. 135.

Method

Method of printing Light Grounds.

Description of the apparatus.

Pl. I, fig. 1. A, the printer's table covered with a soft blanket. B, the woollen cloth sieve on which the colour is laid and spread by a boy (called the tere-boy) with a hair brush. This cloth sieve is laid upon a leather sieve impervious to wet, and it floats upon some gum liquor, in a wooden vessel C.

D, D, two cords of 36 feet long, stretched from the table A to the other end of the room, and kept tight by a weight at B.

F, F, an endless cord, passing round a grooved wheel G under the table, over a pulley H, in the side of the table, and and over another I, at the other end of the room. Its use is to carry the cross-piece K, called the traverse, which is fastened to it.

L, is a wheel fixed on the same axis as the wheel G, but on the outside of the boarding of the table; it has three pegs projecting about four inches from its face. This wheel is moved by the printer setting his foot on one of the pegs.

Fig. 2, is the traverse on a larger scale. M, M, are two pieces of wood connected by a hinge at N, and when closed are retained in that position by a ring O, put over the ends of them: it is connected with the endless cord, by a staple P on one side, and another staple on the other side, and slides along the cords D, D, by means of two pullies R, R.

Method of printing light grounds.

The operation of printing commences by putting one end of the paper to be printed (which is 12 yards long and 23 inches wide) between the divisions of the traverse (fig. 2), and fastening it there by the ring O. The other part of the paper, except what lies on the printing table, is wound round the roller S. The workman takes up the printing block with his right hand, dips the face of it on the woollen cloth in the sieve, which the tere boy had previously spread with colour, and then places the block upon the paper to be printed, giving it two or three smart strokes with a leaden mall held in his left hand; he then removes the block to supply it with more colour from the sieve; and during this operation sets his foot upon the peg in the wheel; and as he recovers his upright position to bring the block over the table, his foot presses the peg

peg down into the position 2, which, by means of the wheel G, endless cord F, and traverse K, draws the paper forward on the table just the proper distance to print again. When the whole piece is printed, the tere-boy goes to the end of the room, loosens the paper from the traverse, and hangs it up to dry in folds, on loose sticks placed across racks attached to the ceiling.

Method of printing Dark Grounds.

Method of printing dark grounds.

The table and sieve for the colour are the same as in printing light grounds. The difference of printing consists in applying the colour from the block upon the table, by means of a lever, instead of striking the block with a mall; the pressure of the lever forcing a greater quantity of colour upon the paper and in a more even manner.

T, the axle of the lever. Y, the arm (15 inches long) to which the power is applied by means of a rope U, fastened to it, which has a treadle W at its end, for the workman to place his foot upon. X, another arm (6 inches long) to which is jointed Y, a long pole, the end of which is applied to the back of the block 3, when the pressure is given.

Z, an arm on the other side of the axle T, to which a weight is hung to balance the pole Y.

Fig. 3, shows a section of the axle T with the arms V and Z projecting from it, and the manner in which the arm X is connected by a joint with the pole Y; the excellence of this principle depends upon the very great increase of power, which is given by bringing the pole near the centre of the joint or axis.

The paper being placed upon the table as in printing light grounds, and the workman having placed his block, furnished with colour, upon the paper to be printed, he puts his foot on the treadle W, attached to the cord U, takes the pole from behind the piece of wood 4, and applies its end upon the block U, and pressing down his foot makes the impression from the block upon the paper. He then lodges the pole behind the piece of wood 4, to be out of the way; he next removes the block to furnish it again with colour, and draws the paper forward for another impression, by the foot-wheel L, as described in the former mode.

IV.

IV.

An Account of the Relistian Tin Mine. By Mr. JOSEPH CARNE, *in a Letter to* DAVIES GIDDY, *Esq. M.P. F.R.S**.

DEAR SIR, *Penzance, April* 22, 1807.

Chlorite shist cemented by crystallized tin.

WHEN I mentioned the occurrence of pebbles of chlorite shist, cemented by crystallized tin, in the Relistian mine, you expressed a wish to receive a particular account of this novel circumstance.

The mine described.

The Relistian mine is nearly on a level with the surrounding country. The lode has been seen at the depth of 12, 25, 50, 65, 75, 81, and 90 fathoms from the surface. It is of different width in different parts; the extreme width is 36 feet, and in this part it is principally worked. As it extends east and west (which is its due course), its width gradually diminishes, till at the distance of 100 fathoms east it is but 5 feet wide. It is composed (excepting the metallic substances) of shist, chlorite, and quartz. In some parts the shist predominates, and in others the chlorite; the quartz is throughout the smallest component part. The engine shaft (see plan A, Pl. I, fig. 4) is situate 8 fathoms north of the widest part of the lode (B). In sinking the shaft a flookan (C), about 2 inches wide, was discovered, bearing a south-east course, which cut the lode at an angle of 45°; and heaved and disordered it.

At the depth of 12, 25, and 50 fathoms, nothing was discovered in the lode but the cavities from which the ore had been taken away during the former period of working the mine.

At 65 fathoms in depth were found, close to the flookan, a great number of angular fragments of shist, cemented by the same substance.

Flookan divided into four different branches, with a body of pebbles between them,

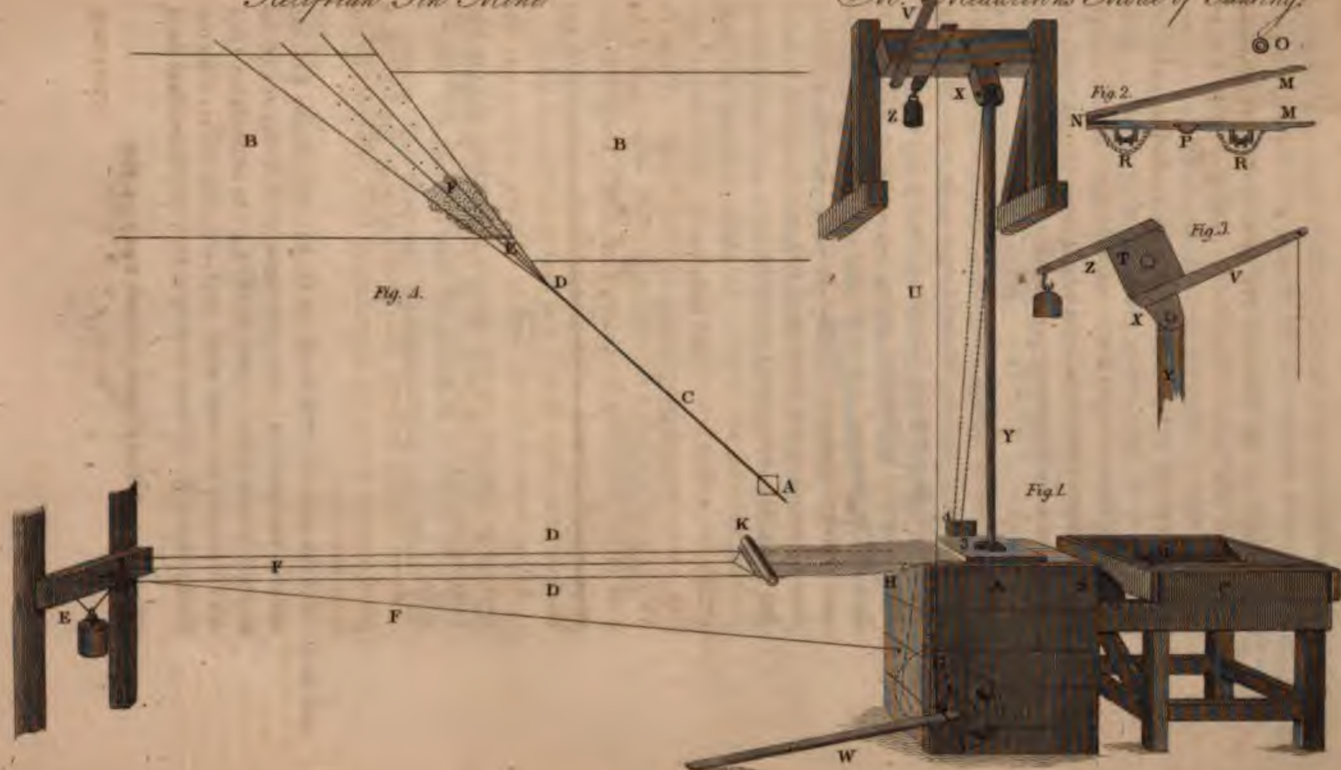
At the depth of 75 fathoms the flookan (C) became 4 inches wide in the shaft (A), and continued of that size for 10 fathoms; it then became divided into 4 parts or branches (D), each diverging from its former course, and in this state it continued through the lode (B), of which the first 3 feet were

* Philos. Trans. for 1807, Part II, p. 298.

composed

Relistian Tin Mine

Mr. Middleton's Mode of Painting.



composed of copper pyrites (E), and then was discovered a body of pebbles (F), nearly 12 feet square, extending in width to the extreme branches of the flookan. In this part of the lode the shist greatly predominates; of course the pebbles are generally composed of shist, cemented in some parts by the same substance or chlorite, in others by oxide of tin, which is generally crystallized, and in some of the crevices there is a little copper pyrites. It is singular, that a few pebbles (perhaps not more than half a score) were found of quite a different nature from the others; they were composed of tin in quartz coated with chlorite. chiefly shist, cemented by shist, chlorite, or oxide of tin.

The pebbles did not continue in a body to the height of more than 2 fathoms; but scattered bunches, and single pebbles, were found 4 fathoms above and 6 fathoms below the place in which they were at first discovered. It is only necessary to add, that the lode has since been worked 15 fathoms deeper than where the pebbles occurred; it there consists for the most part of chlorite formed in a regular manner; not the least trace of pebbles is to be seen, nor indeed of any disturbance in the strata.

I am, dear Sir,

very sincerely yours,

Penzance, Cornwall. JOSEPH CARNE.

V.

An Analysis of the Waters of the Dead Sea and the River Jordan. By ALEXANDER MARCET, *M. D. one of the Physicians to Guy's Hospital. Communicated by* SMITHSON TENNANT, *Esq. F. R. S**.

THE Dead Sea, or Lake Asphaltite, is situate in the southern part of Syria, near Jerusalem, and occupies an extent of about 60 or 70 miles in length, and from 10 to 20 in breadth. This lake has been from time immemorial celebrated on account of the intense saltness of its waters, which Dead Sea.

* Philos. Trans. for 1807, Part II, p. 296.

is

is such as to prevent either animals or vegetables from living in it, a peculiarity from which it has derived its name. It appears, that this saline quality has existed in the earliest ages; for independently of the frequent allusions made to it in the Scriptures, we find it described by several ancient authors, amongst others by Strabo*, who wrote during the reign of Augustus, by Tacitus†, and by Pliny‡. Amongst modern travellers, Pococke§, Volney‖, and others, have noticed and described this singular spot.

Only analysis of it.

But although the most obvious peculiarities have for a long time been in some degree known, the only chemical analysis I have been able to find on record is that which was published in the "*Mémoires de l'Académie des Sciences*" for the year 1778, by Messrs. Macquer, Lavoisier, and Sage. The names of Lavoisier, and of his two distinguished associates, might appear to render any further investigation of the nature of this water superfluous; but whoever has perused the paper in question must be convinced, that these gentlemen, however correct in their general statements, neither attained that degree of accuracy of which modern analysis is susceptible, nor did they bestow on the subject that share of attention, which is indispensable in minute analytical experiments.

Water brought home by Mr. Gordon.

The gentleman to whom I am indebted for the specimen of the water of the Dead Sea, which is the subject of this pa~~per, is Mr. Gordon of Clunie,~~ who recently travelled in that country, and undertook, not without some difficulty and danger, an excursion from Jerusalem to this remarkable lake. There he himself filled and brought to Sir Joseph Banks a phial, containing about one ounce and a half of this water, carefully corked, and in a state of perfect preservation. The same gentleman brought also in another phial, somewhat larger, a specimen of the River Jordan, which runs into the Dead Sea, without having any outlet, so that the river might be expected to hold in solution ingredients analogous to those of the Lake itself. These specimens Sir Joseph put into the hands of Mr. Tennant, for examination. But knowing that I

* Strabonis Geogr. vol. ii, p. 1107.
† Tacitus, lib. v, *Hist.* cap. vi.
‡ Plinii lib. v, cap. xv, and xvi.
§ Pococke's *Travels* in 1743, ii, p. 34.
‖ Volney, i, 281.

was

was engaged in similar researches, Mr. Tennant was so obliging as to entrust me with this analysis, and to afford me frequent opportunities of availing myself of his assistance in the course of the inquiry.

Being possessed but of a small quantity of this water, a further supply of which could not easily be procured, I was anxious not to waste any considerable portion of the specimen by preliminary trials. With this view, I began by making a variety of comparative experiments on artificial solutions, in order to ascertain the accuracy of different modes of operating; and knowing by Lavoisier's analysis, and also by the general effects of reagents applied to minute quantities of the water, what were the principal ingredients which I might expect to find in it, I made solutions, the contents of which I had previously ascertained with precision, so that by analysing these solutions in different ways, I had an opportunity of judging of the degree of accuracy that could be expected from a variety of methods. Some of these trials I shall briefly relate; for although not strictly belonging to the particular analysis in question, yet I conceive, that they may be of some general use, in pointing out the most eligible method to be pursued in inquiries of this kind. Indeed it must be confessed, that the minute chemical examination of any individual substance requires so much time and patience, that to obtain a knowledge of that substance only would seldom appear a sufficient inducement to such a laborious undertaking, was it not always more or less connected with other useful collateral objects. **Preliminary observations.**

Sect. I.

General Properties of the Dead Sea.

1. One of the most obvious peculiarities of the Dead Sea-water, is its specific gravity, which I found to be 1·211, a degree of density scarcely to be met with, I believe, in any other natural water. The circumstance of this lake allowing bodies of considerable weight to float upon its surface was noticed by some of the most ancient writers. Strabo, amongst others, states that men could not dive in this water, and in going into it, would not sink lower than the navel; and Pococke, who bathed **General properties of the water.**

bathed in it, relates that he could lie on its surface, motionless, and in any attitude, without danger of sinking. These peculiarities, which I at first suspected of being exaggerated, are fully confirmed by Mr. Gordon, who also bathed in the lake, and experienced all the effects just related.

2. The water of the Dead Sea is perfectly transparent, and does not deposit any crystals on standing in close vessels.

3. Its taste is peculiarly bitter, saline, and pungent.

4. Solutions of silver produce from it a very copious precipitate, showing the presence of marine acid.

5. Oxalic acid instantly discovers lime in the water.

6. The lime being separated, both caustic and carbonated alkalies readily throw down a magnesian precipitate.

7. Solutions of barytes produce a cloud, showing the existence of sulphuric acid.

8. No alumine can be discovered in the water by the delicate test of succinic acid combined with ammonia.

9. A small quantity of pulverized sea salt being added to a few drops of the water, cold and undiluted, the salt was readily dissolved with the assistance of gentle trituration, showing that the Dead Sea is not saturated with common salt.

10. None of the coloured infusions commonly used to ascertain the prevalence of an acid or an alkali, such as litmus, violet, and turmeric, were in the least altered by the water.

Sect. II.

Preliminary Experiments to ascertain the Composition of the Salts concerned in this Analysis.

Comparative experiments.

Having satisfied myself by these preliminary experiments, that the Dead Sea contained muriate of lime, muriate of magnesia, and selenite, and having no doubt both from the taste of the water, and from Lavoisier's statement*, that it contained also common salt, I proceeded to the comparative experiments above mentioned.

The first indispensable step was to ascertain with accuracy the proportions of acid and base in the three muriates just named.

* Macquer, Lavoisier, and Sage, discovered the three muriates, but overlooked the small quantity of selenite.

named. This I had already done in the course of a more general inquiry, which I began some time ago in conjunction with Mr. Tennant, and which has been of great use to me on the present occasion. But as the particulars of that series of experiments may probably be published at some future period, I shall now confine myself to such general statements as immediately belong to my subject.

1. The composition of muriate of lime was ascertained by pouring a known measure of muriatic acid on a piece of pure marble of known weight, and more than sufficient to saturate the acid. The remaining portion of marble being then weighed, and the solution evaporated and heated to redness, the proportions of acid and earth were easily deduced. But in order to draw such an inference, it was necessary to ascertain with precision the quantity of pure lime in a given weight of marble, which, from a number of experiments performed with great care by Mr. Tennant and myself, appeared to be 56·1 parts of lime in 100 of marble. From a great variety of trials, made with considerable attention, and with due allowance for any accidental circumstances, muriate of lime appeared to consist of 50·77 parts of lime, to 49·23 of muriatic acid.

Muriate of lime consists of 50·77 lime, 49·23 acid.

Marble contains 56·1 lime

2. To ascertain the proportions of earth and acid in muriate of magnesia, required a synthetic process somewhat different. To a known weight of pure magnesia perfectly calcined, a known quantity of acid* was added, and after the whole of the magnesia was dissolved, the remaining portion of acid was saturated by marble. From the loss sustained by the marble, and the known proportions of acid and magnesia used, the composition of muriate of magnesia (supposed perfectly free from water) was deduced, and the proportions resulting from several careful trials were 43·99 parts of magnesia, to 56·01 of muriatic acid.

Muriate of magnesia contains of magnesia 43·99 acid 56·01.

3. Muriate of soda was analysed by various methods. But the

Muriate of

* By a *known quantity* of acid is meant as much acid as will dissolve a known weight of marble. In all these experiments the quantities of acid were not weighed, but measured by means of a peculiar apparatus, and the real weights or intrinsic quantities of acid, corresponding to the measures in question, were easily deduced from the results above mentioned.

soda, 54 soda, 46 acid.

the only one which I shall now relate consisted in precipitating the acid by a solution of silver from a known weight of muriate of soda, and inferring the proportion of acid and alkali from the quantity of luna cornea obtained. This however required a previous exact knowledge of the proportions of acid and silver in luna cornea. In order to ascertain this point, a known quantity of acid was precipitated by nitrate of silver, and the weight of the luna cornea, after being melted and heated to redness, indicated 19·05 parts of acid to 80·95 of oxide of silver. The composition of common salt, calculated from these data, proved to be 46 parts of acid to 54 of soda.

Muriate of silver, 80·95 oxide, 19·05 acid.

SECT. III.

Comparative Analysis of artificial Solutions.

Artificial solutions of these salts analysed.

I shall not enter into all the particulars of the various analyses of artificial solutions, resembling the water of the Dead Sea, which directed me in the choice of the method which I ultimately adopted. But it may be proper to state, in a summary manner, the principal means which were tried, and their respective defects and advantages.

These artificial mixtures all contained the three muriates above mentioned, but in each of them the small quantity of selenite was altogether disregarded.

1. The first of these solutions was evaporated to dryness, and the residue exposed for near an hour to a red heat in a platina crucible pretty closely covered. The object of this was to drive off the acid from the magnesia (muriate of magnesia being decomposable by heat), and after separating this earth from the other salts by means of distilled water, to precipitate the lime by carbonate of ammonia, and to obtain the muriate of soda by evaporation to dryness. But I soon found, that the complete decomposition of muriate of magnesia by heat, under these circumstances, was extremely difficult, if not impossible; and accordingly the results obtained from this method indicated considerably less magnesia and proportionally more lime, than the solution really contained. The quantity of common salt was tolerably accurate.

Heat did not completely decompose the muriate of magnesia.

2. From

2. From another similar solution the lime was precipitated by oxalate of ammonia; the magnesia was separated by heat in an *open* crucible, and the common salt was obtained, as before, by evaporation and exposure to a low red heat. The result was satisfactory both as to the lime and magnesia; but as the separation of the latter could only be completed by long continued heat, in an open vessel, I found the muriate of soda materially reduced by sublimation, and was therefore obliged to abandon this mode of proceeding. The muriate of soda diminished by sublimation.

3. From a third artificial solution, the lime was precipitated by oxalate of ammonia, the magnesia by carbonate of ammonia recently prepared, and the sea salt was obtained as usual by evaporation and desiccation in a low red heat. The object of this mode of operating was to supersede the necessity of applying a red heat in the first instance. But I was again disappointed; for the magnesia was but imperfectly precipitated; and in order to separate the last portions of this earth, it was necessary to calcine the last residue containing the muriate of soda, which gave rise to the same objections as in the former experiments. A 3d trial defective.

4. The last and most successful method consisted in dividing the artificial solution into two portions. From one of these the muriatic acid was precipitated by nitrate of silver, and its quantity ascertained. From the other the lime was separated by oxalate of ammonia, and the magnesia by caustic potash*; and the respective portions of acids belonging to each of these earths being calculated, the quantity of muriate of soda was inferred from the remaining quantity of acid. Most successful method.

This method afforded remarkably accurate results. The only objection to it seems to be, that the muriate of soda being only estimated, and not actually obtained, if any errour be made either in the estimation of the acid or in the separation of the lime and magnesia, these errours must also ultimately affect the computation of the muriate of soda, without allowing any immediate means of detecting them. Only objection to it

* Or, by carbonate of ammonia. In this case the precipitation of magnesia is not so perfect; but the precipitate falls down more quickly, and the separation of any remaining portion of this earth may be ultimately completed by heat.

This

in great measure removed. This objection, however, is in a great degree removed, by a comparison of the two portions of the solution, from one of which the common salt can be obtained undecomposed; and the present method has this additional advantage, that the quantity of acid is a sort of check, which, when connected with some other point of comparison, prevents any gross errour in the computation of the earths from escaping notice.

This plan being very similar to that which I actually followed in the analysis of the water of the Dead Sea, it may be worth while to mention the summary results of the comparative experiments which decided me in its favour.

The artificial solution contained:

Actual contents of the solution.

	Salts.		Acid.
Muriate of lime······	8·17 grains		4·02 grains.
Muriate of magnesia··	26·10	=	14·62
Muriate of soda······	25·00	=	11·50
	59·27*	=	30·14

And the contents inferred by the foregoing method, were:

Contents given by the analysis.

	Salts.		Acid.
Muriate of lime ······	8·14	=	4·01 grains.
Muriate of magnesia ··	25·62	=	14·35
Muriate of soda ······	25·47	=	11·72
	59·23	=	30·08

SECT. IV.

Analysis of the Dead Sea Water.

Analysis of the Dead Sea water. I now come to the actual examination of the water of the Dead Sea, the particulars of which will be found much shortened by the preceding observations.

* These happened to be very nearly the real proportions of salts in the Dead Sea; yet this coincidence was a matter of mere accident; for when I mixed up the ingredients, I was led to suppose from Lavoisier's paper, that their proportion in the Dead Sea was very different from that which I afterward ascertained.

1. 20

1. 20 grains of this water (the whole supply of which amounted only to 540 grains) were put into a glass capsule, and slowly evaporated in a water bath, by means of an appropriate apparatus, the temperature of the capsule being constantly kept within 5 degrees of 180°. The object of this experiment was simply to know the weight of the solid contents of the water, dried under various degrees of heat, and to observe the appearances produced by evaporation. After a few hours, and when the residue had ceased to lose weight, the saline mass, whilst still warm, appeared in the form of a white semitransparent incrustation, which yielded to the touch, being soft, and of a pulpy consistence. In cooling it became hard, and of a much more opaque white colour. When examined with attention, the borders of this mass were found covered with small cubic crystals, and the same appearance was observed, though less conspicuously, in the centre under the saline incrustation, when in the state of semifusion just described. On standing in the air for some time, the white opaque mass gradually absorbed water from the atmosphere, and returned to a liquid state. The 20 grains of the water, thus evaporated and dried at 180°, weighed, whilst still warm, 8·2 grains. By evaporation.

2. The same saline mass, being afterward exposed in a sand bath to the temperature of 212° Fahrenheit, was reduced to 7·7 grains. Hitherto not the least smell of muriatic acid was perceived, nor did any decomposition appear to take place.

3. But having raised the heat about 15° higher, the residue, after a few minutes, was found reduced to 7·4 grains; and on redissolving it, a few insoluble white particles appeared floating in the solution, showing an incipient decomposition in the muriate of magnesia.

It appears from these experiments, that 100 parts of the Dead Sea water yield 41 of salts dried at 180°, and 38·5 dried at 212°*. What proportion these quantities bear to the

* If the quantity of materials upon which these results are founded should appear too small, I would observe, that, if the bulk of salt be considerable, it is impossible to dry it accurately, owing to the crust which forms on the surface, and prevents the escape of moisture. But at

the same salts, when perfectly deprived of water, will be seen from the subsequent results. I now pass on to the chemical examination of the water.

By nitrate of barytes.

4. To 100 grains of the Dead Sea water a few drops of muriate of barytes being added, a precipitate was obtained, which, after being well washed and exposed to a low red heat on a piece of laminated platina, weighed 0·09 grain, which, allowing for the unavoidable loss attending the manipulation of such very minute quantities, may safely be called 0·1 grain. This residue, on being heated with fluat of lime, instantly ran into a globule, and was evidently sulphate of barytes.

By muriate of silver.

5. To another portion of the Dead Sea water, weighing 250 grains, a solution of nitrate of silver being added till it ceased to produce any precipitate, a quantity of luna cornea was obtained, which after careful edulcoration and exposure to a red heat, weighed 163·2 grains, a quantity equivalent, according to the proportions above stated (sect. II, 3), to 31·09 grains of real acid.

Muriate of ammonia added.

6. To the remaining solution a little muriate of ammonia was added, in order to remove the unavoidable small excess of silver, and this new precipitate was separated and well edulcorated.

Oxalate of ammonia.

7. The clear fluid, which had been much increased in bulk by these edulcorations, being concentrated to about 3 ounces, a strong solution of oxalate of ammonia, warm, but not nearly boiling*, was added to it, by which a precipitate was obtained, which collected and washed with the usual precautions, and after deducting 0·076 grains of lime† for

at any rate no perfect accuracy can be relied on respecting this kind of limited desiccation, as its completion depends in a great degree on the shape of the vessel, the thickness of the stratum of salt, &c.

* The precipitates of lime by oxalate of ammonia subside more readily, if the solutions be used warm; but when concentrated and heated to the boiling point, this test acts also in some degree on magnesia, a circumstance which in the present instance was to be particularly avoided.

† The proportion of lime in selenite, and of acid in sulphate of barytes, are taken from a paper of Mr. Chenevix, in Nicholson's Journal, Vol. II, in which they are stated to be 56·4 of lime in 100 parts of selenite, and 24 parts of acid in 100 parts of sulphate of barytes.

the

the 0·136 grains of selenite belonging to 250 grains of the water, yielded 4·814 grains of pure lime = 4·66 grains acid = 9·48 grains muriate of lime.

I should not omit mentioning, that the method which I used in all my experiments to ascertain the quantity of pure lime in oxalate of lime, consisted in driving off the oxalic acid by a low-red heat, and adding to the calcareous residue, then converted into a subcarbonate, a known quantity of muriatic acid more than sufficient to dissolve the whole lime. A piece of marble of known weight was afterwards added to take up the excess of acid, and from these data the quantity of lime was calculated with great precision.

8. The clear solution containing nitrate of magnesia, nitrate of soda, and a small excess both of oxalate and muriate of ammonia, and amounting in bulk to about 4 ounces, was exposed to the heat of a lamp for concentration; but in a few minutes the mixture became turbid and began to deposit a white powder, which, from former observations, I supposed to be oxalate of magnesia. To this solution concentrated to between 2 and 3 ounces, and still warm, I added carbonate of ammonia with excess of pure ammonia. A considerable precipitation immediately appeared, and the mixture became opaque and milky. The next morning, however, the fluid had become quite transparent, and instead of a white impalpable precipitate, I found clusters of perfectly pellucid crystals spread over the bottom of the vessel, with distinct interstices between them.

Subcarbonate of ammonia.

This salt was no doubt an ammoniaco-magnesian carbonate; and the remaining solution, although still containing, as will presently appear, a vestige of magnesia, was so far free from it, as not to have its transparency disturbed by caustic potash. These crystals, after being well washed in distilled water, were exposed to a gentle heat to drive off the ammonia, in consequence of which they crumbled down into a white impalpable powder, exactly resembling common carbonate of magnesia. This powder being then treated, and its quantity estimated, in a way similar to that which had been employed with the lime; and being increased by the addition of about 0·5 of a grain of a similar precipitate (which had escaped the action of the carbonate of ammonia

and was obtained from the last remaining solution by evaporation and calcination), amounted to 11·10 grains of pure magnesia = 14·15 grains of muriatic acid = 25·25 grains of muriate of magnesia.

9. The muriate of soda was next estimated from the 12·28 grains of muriatic acid found to remain after substracting the sum of the two portions (4·66 grains and 14·15 grains) belonging to the lime and magnesia, from the 31·09 grains, or sum total of acid. These 12·28 grains gave according to the proportions before mentioned (sect. II, 3) 26·69 grains of muriate of soda.

10. From these several results brought into one view, and the salts being supposed heated to redness, 250 grains of the Dead Sea water appear to contain,

Contents of 250 grains of the water,

	Salts.		Acid.
Muriate of lime ······	9·480 grains		4·66 grains
Muriate of magnesia ··	25·25	=	14·15
Muriate of soda ······	26·695	=	12·28
Sulphate of lime ······	0·136		
	61·561		31·09

or of 100. And therefore 100 grains of the same water would contain,

	Grains.
Muriate of lime ···············	3·792
Muriate of magnesia ···········	10·100
Muriate of soda ···············	10·676
Sulphate of lime ···············	0·054
	24·622

Sect. V.

2d. analysis,

Second Analysis of the Dead Sea Water by a Method somewhat different from the former.

by a somewhat different mode.

In the mode of proceeding just related some small loss in the earths might naturally be suspected to have taken place in consequence of the previous separation of the acid and indispensable edulcorations. Besides, the muriate of soda being necessarily decomposed by the first part of the process, the analysis could not have been considered as quite satisfactory,

satisfactory, had not the common salt been procured unaltered by some other process.

1. In order to obtain these points, 150 grains of the water were treated, with regard to the lime and magnesia, exactly as in the former analysis; but in this case, the acid, instead of being actually separated by silver, was only calculated from the former estimation (sect. IV. 5).

2. The result proved perfectly agreeable to my expectation. It yielded a little more lime and magnesia than the former analysis, but this excess was scarcely perceptible. With regard to the muriate of soda, I was able actually to procure by evaporation as much as 13·1 grains of this salt, the actual quantity of which, inferred as in the preceding analysis, was 15·54 grains, a difference easily accounted for by the necessity of heating the salt to redness for its ultimate separation.

3. On summing up the contents of these 150 grains of the water, they appeared to be as follow:

Contents of 150 grains, by this analysis.

	Salts.	Acid.
Muriate of lime	5·88 grains	2·89 grains.
Muriate of magnesia	15·37 =	8·61
Muriate of soda	15·54 =	7·15
Selenite	0·08	
	36·87	18·65

And consequently the proportions of these salts in 100 grains of the water would be:

Proportions in 100.

	Grains.
Muriate of lime	3·920
Muriate of magnesia	10·246
Muriate of soda	10·360
Sulphate of lime	0·054
	24·580

The coincidence of these results with those of the former analysis was such as I could scarcely have expected to increase by further trials. The last statement, however, I consider as the most accurate of the two.

It

General result.

It may therefore be stated in general terms, that the Dead Sea water contains about one fourth of its weight of salts supposed in a state of perfect desiccation; while, as I observed before, if these salts be only desiccated at the temperature of 180°, they will amount to 41 per cent of the water. This great difference between the two states of desiccation depends on the great affinity which muriates, particularly that of magnesia, have for water. Muriate of soda is scarcely at all concerned in this difference: for I found, not without surprise, that 100 grains of artificial cubic crystals of muriate of soda, being fused and heated to redness in a platina crucible, lost at most half a grain.

Proportions of Macquer and Lavoisier.

In the analysis of Macquer and Lavoisier, the solid contents of the Dead Sea are estimated at about 45 per cent of the water, and in the proportions of nearly 1 part of common salt to 4 of muriate of magnesia, and 3 of muriate of lime; proportions widely different from those which I had obtained. But their mode of operating, which they candidly relate, was so evidently inaccurate with regard to the separation and desiccation of the salts, and in general so deficient in the estimation of quantities and proportions, that these eminent chemists cannot be considered as having aimed, in this instance, at any thing like an exact analysis.

It may be observed also, that these gentlemen found the specific gravity of the water 1·240 instead of 1·211, as I have stated it to be; but it appears, that their specimen had suffered some evaporation previous to their experiments, since they found crystals of common salt in one of their bottles, which could not have happened without evaporation.

The proportions above perhaps rather too small.

Besides, the specimen which I examined was, I understand, brought from a part of the lake not more than two miles distant from the mouth of the Jordan, a circumstance which may perhaps account for its being somewhat more diluted, than it might be found in other parts.

Sect. VI.

Analysis of the Water of the River Jordan.

Water of the Jordan.

As I had scarcely two ounces of this water, and as it contained but a very small proportion of saline ingredients, it would

would have been in vain to aim at analysing it with strict accuracy. Yet I thought it worth while to endeavour to form as exact an estimation of its contents as I could, on account of its connection with the Dead Sea, into which, as was observed before, it pours its waters, and appears to remain in a stagnating state. This specimen was brought from a spot about three miles distant from that where the river enters the Dead Sea.

Apparently pure,

From the perfect pellucidity of this water, its softness, and the absence of any obvious saline taste, I was led to suppose, that it was uncommonly pure, and could in no degree partake of the peculiar saline qualities of the Dead Sea. But I was soon induced to alter my opinion by the following results.

but analogous to that of the Dead Sea except in strength.

1. The same chemical reagents, as were used to ascertain the general properties of the Dead Sea water, being applied to this, produced analogous effects. The same three muriates and even the vestige of selenite were distinctly discovered; and this resemblance became more striking in proportion as the water was concentrated by evaporation.

Apparently contains $\frac{1}{300}$ only of its solid contents.

2. 500 grains of this water being evaporated at about 200°, the dry residue weighed exactly 0·8 of a grain. This makes the solid ingredients amount only to 1·6 grain in 1000 grains of the water, a singular contrast with the Dead Sea, which contains nearly 300 times that portion of saline matter. As the water was concentrating, a few white particles were perceived on its surface, and a few others gradually subsided. When dried, the residue appeared in the form of a white incrustation, the upper edge of which exhibited great numbers of very minute crystals, which from their saline taste, and their cubic shape, discoverable by the aid of a microscope, were evidently common salt.

3. Distilled water being thrown on this residue, a minute portion of it remained undissolved, and on pouring an acid on this substance, a distinct effervescence was produced, showing the presence of carbonate of lime.

4. From the clear fluid a precipitate was obtained by oxalate of ammonia, which, dried but not calcined, weighed 0·12 of a grain.

5. From

5. From the remaining clear solution a magnesian precipitate was produced by ammonia and phosphoric acid, which, after driving off the ammonia by heat, weighed 0·18 of a grain.

6. The solution had suffered too many alterations to allow me to separate, with any degree of accuracy, the muriate of soda; but from a variety of circumstances, I thought it not unlikely, that it would have been found pretty nearly in the same proportions, with respect to the other salts, as it exists in the Dead Sea.

The Dead Sea perhaps the same water concentrated by evaporation.

The inference I drew from this was, that the River Jordan might possibly be the source of the saline ingredients of the Dead Sea, or at least that the same source of impregnation might be common to both. This inquiry, however, would require a much more correct knowledge both of the proportions of the salts, and of local circumstances, than I have been able to obtain.

VI.

An Account of the Measurement of an Arc on the Meridian of the Coast of Coromandel, and the Length of a Degree deduced therefrom in the Latitude of 12° 32′. *By Brigade Major* WILLIAM LAMBTON.

(Concluded from Vol. XIX, p. 317.*)*

Reductions of the hypothenuses.

THE reductions from the hypothenuses to bring them to the horizontal level were made by numbering the feet from the old chain as they were measured, viz. by calling 32 chains 3200 feet, which would be 3200·115 feet by the new chain; but this would produce no sensible errour in the versed sign of a very small angle, and on that account these decimals were not taken into the computation, which was thought less necessary, since the whole deduction did not amount to three inches. Neither was any notice taken of the different heights of the hypothenuses or levels one above another, as that difference was too trifling to affect a length of thirty or forty chains. The base has therefore been considered

sidered at the same distance from the centre of the earth, before it was reduced to the level of the sea, and the perpendicular height of the south extremity, which I have considered as nearly the general height, has been taken for that purpose. That perpendicular height was obtained by comparing the south with the north extremity, and the height of the latter was determined by observations made at the race-stand and on the sea-beach, where allowance has been made for the terrestrial refraction. The following is the manner in which it has been determined:

Determination of the height above the sea.

On the top of the race-stand, the under part of the flag on the beach was observed to be depressed 9′ 30″; and at the beach, the top of the race-stand was elevated 7′ 15″. When the instrument was on the platform of the race-stand, the axis of the telescope was on a level with the top of the railing, which was observed from the beach. But at the beach the axis of the telescope was four feet below the part of the flag which had been observed.

The horizontal distance from the station on the stand to that on the beach is = 19208 feet. Then as 19208 : 4 :: Rad : tan. 43″, which must therefore be added to the observed depression of the flag. Hence $9' 30'' + 43'' = 10' 13''$ is the depression of the axis of the telescope on the beach, observed from the race-stand.

Now the station on the beach is nearly at right angles to the meridian, therefore, by allowing 60957 fathoms to the degree, 19208 feet will give an arc of 3′ 9″ very nearly, which is the contained arc. And the difference between the depression and elevation being 2′ 58″, we have $\frac{3' 9'' - 2' 58''}{2} = 5''{\cdot}5$ for the terrestrial refraction. Hence, since the observed elevation of the stand, *plus half* the contained arc, would give the angle subtended by the perpendicular height of the stand above the telescope at the beach, were there no refraction, we shall have $7' 15'' + \frac{3' 9''}{2} - 5''{\cdot}5 = 8' 44''$ for the true angle subtended by the perpendicular height, which being taken as tangent to the horizontal distance and radius, we have R : tan. 8′ 44″ :: 19208 : 48·797 feet the height required. But the axis of the telescope on the beach was determined, by levelling down

down to the water, to be 21·166 feet above the sea. Which, added to the above, give 69·963 feet for the perpendicular height of the top of the stand above the level of the sea.

Now the top of the race-stand was determined by levelling to be 31·25 feet above the north extremity of the base; which, taken from the other, leaves 38·713 for the north extremity of the base above the sea, which extremity being, by the table, 22·96 feet above the south extremity, we shall have 15·753 feet from the perpendicular height of the south extremity of the line above the level of the sea; and from this height the length of the base has been reduced.

The angles of elevation and depression were taken by the circular instrument, from a mean of several observations, and the errour of collimation was corrected by turning the transit over, and the horizontal plate half round. But the weather was rather dull during the whole of these operations.

Major Lambton then proceeds to give the particulars of the measurement of his base line, commencing in lat. 13° 00′ 29·59″ N., and extending 40006·4418 feet south-westerly, making an angle of 10′ 36″ with the meridian.

Commencement of the operations from the base. The large theodolite.

Properest stations selected.

After the completion of the base line, there remained nothing of importance to be done until I received the large instrument, which arrived in the beginning of September. I had however made an excursion down the sea coast, as far as Pondicherry, for the purpose of selecting the properest stations for determining the length of a meridional arc. This and the measurement of a degree at right-angles to the meridian I considered as the first object of this work: I accordingly lost no time in proceeding to accomplish these desiderata.

Theodolite.

The instrument above alluded to was made by Mr. Cary, and is in most respects the same as that described by General Roy in the Philosophical Transactions for the year 1790, with the improvements made afterwards in the microscopes,

scopes, and in an adjustment to the vertical axis, by which the circle can be moved up or let down by means of two capstan screws at the top of the axis. These are mentioned in the Philosophical Transactions for 1795, in the account of the trigonometrical survey. By sinking the circle on the axis, it is better adapted for travelling, and when the microscopes are once adjusted to minutes and seconds, on the limb of the instrument, the circle can always be brought back to the proper distance from them. Great attention however is necessary in bringing the axis down, so that the wires in each microscope being fixed at opposite dots on the limb, they may coincide with the same dots when the circle is turned half round, or made to move entirely round, and in a contrary direction to what it had been moved before; which latter method has been recommended by the maker. This circumstance respecting the axis should be most scrupulously attended to before the adjustment of the micrometers begins, so that when by arranging the lenses in such a manner that ten revolutions of the micrometer may answer to ten minutes on the limb, and therefore one division to one second, the circle can always be brought to its proper height, by trying the revolutions of the micrometer.

It has however been found from experience, that unless in cases of very long and troublesome marches, it is not necessary to sink the axis. The carriage being performed altogether by men, there is not that jolting which any other mode of conveyance is subject to, and as I found, that a considerable time was taken up in adjusting the axis before the revolutions of the micrometers could be brought to their intended limits, I therefore laid it aside, unless under the circumstances above mentioned.

Semicircle of the transit telescope.

The semicircle of the transit telescope is graduated to 10′ of a degree in place of 30′, which was the case with the semicircle described by General Roy, and the micrometer to the horizontal microscope applied to this semicircle, making one revolution in two minutes, and five revolutions for ten minutes on the limb; and the scale of the micrometer being divided into sixty parts, each part is therefore two seconds of the circle.

Errour of the semicircle.

A number of experiments have been made for determining

ing the errour of the semicircle, and to ascertain the place of the fixed wire in the horizontal microscope, so as to divide the errour. It has appeared in the event, that the telescope being in its right position, (that is, when the limb and microscope were on the left hand,) and the fixed wire placed at zero on the semicircle, when the circle or limb of the theodolite was turned 180° in azimuth, and the telescope turned over, the fixed wire was then distant from zero on the opposite part of the arc by a mean of a great many observations 2′ 57″, the half of which is therefore the errour. This half was carefully set off from zero by the movable micrometer wire, and the fixed one brought to coincide with it. On the right application of this errour, there will be 1′ 28″·5 to add to the elevations and subtract from the depressions. The observations for determining this quantity were repeated at different times, and under the most favourable circumstances; the adjustments of the whole instrument being frequently examined, and the level applied to the telescope reversed at most of the observations. For the line of collimation, as these corrections depend on having a well defined object, I fixed a bamboo upwards of a mile distant from the observatory tent, and tied round it several narrow stripes of black silk, one of which was near the horizontal wire when the axis of the telescope intersected the staff after being brought to a level by the bubble. Then the instrument being adjusted, and the telescope directed to the bamboo being perfectly level, and the wire of the micrometer in the piece brought to the intersection of the cross wires, the angular distance to the mark on the bamboo was measured by the runs of that micrometer, and the wire brought back to the point of intersection of the other wires. The circle was then turned half round and the telescope reversed or put again into the same Ys. The levelling adjustment was then made, and the angular distance from the intersection of the wires to the black mark again taken, half the difference between which and the former was of course the errour of collimation. This errour was repeatedly reduced till it became very small, half by the finger screw of the clamp to the semicircle, and half by the adjusting screws to the levelling rods. After that, the remaining errour

Line of collimation.

rour was repeatedly examined and found to be 2″·36 to be substracted from the elevations and added to the depressions when the telescope is in the ordinary position, or when the semicircle and microscope are on the left hand; but *vice versa* when in the contrary position. These errours of the semicircle and line of collimation being opposite, the result from comparison will be, "That when *elevations* or *depressions* are taken with the semicircle, 1′ 26″ must be added to the *former*, and substracted from the *latter*."

And that when the elevations and depressions are taken by the micrometer in the eye piece 2″·36 must be *deducted* from the *elevations* and added to the depressions.

The micrometer in the focus of the eye-glass of the transit telescope is the same in all respects as the one mentioned by General Roy, that is to say, the circle or scale is divided into one hundred divisions, and there is a nonius fixed to the upper part of the telescope, which defines the revolutions of the micrometer as far as ten for the elevations, and ten for the depressions. Several experiments were made with the same marked bamboo, for ascertaining the value of these divisions, and it was found, that seven revolutions and 61·4 divisions were equal to ten minutes on the limb of the semicircle, so that one division was equal to ·788 of a second. Micrometer.

Having given tables of all the angles, Major Lambton adds. The angles have been taken with much care, and I believe with as much accuracy as the nature of such a process admits of; difficulty, however, very frequently arose from the haziness of the weather, which rendered the objects at the very distant points extremely dull, and occasioned some irregularity in the angles. Whenever that happened, the observations were often repeated, and in case any one, in particular, was different from the other so much as ten seconds, it was rejected till the three angles of the triangle had been observed. If the sum of these angles was near what it ought to be, no further notice was taken of it; but should the sum of the three angles be nearer the truth by taking it into the account, and that there appeared an irregularity in the other two observed angles, I have made it a rule to take each observed angle as a correct one, and divide Remarks on the angles taken.

divide the excess or defect between the other two, and then compute from the given side the other two sides; and after doing the same thing with each of the angles successively, a mean of the sides thus brought out was taken, which, to certain limits, will always be near the truth. I then varied the selection of the observed angles, rejecting such as I had reason to doubt; and by correcting them, and computing the two required sides of the triangle, those which gave the sides nearest to what had been brought out by the other method were adopted, let the errour be what it would. This, however, has rarely happened; and when it did, great precaution was used; and no angle was rejected, without some reason appeared to render it doubtful.

In correcting the observed angles to obtain those made by the chords, I have used the formula given by the Astronomer Royal, in his demonstration of M. de Lambre's problem, which appears in the Philosophical Transactions for 1797. The spherical excess is of course had from the well known method of dividing the area of the triangle in square seconds, by the number of seconds in the arc equal to radius, where the number of feet in a second may be had by using the degree as has been commonly applied to the mean sphere, or the mean between the degree on the meridian and its perpendicular. This being of no farther use than to check any errour that might happen in computing the corrections for the angles.

Observations by the Zenith Sector for the latitude of Paudree *station, and the station near* Trivandeporum; *and the length of the celestial arc.*

Zenith sector. The zenith sector, with which these observations have been taken, was made by Mr. Ramsden, and is the one alluded to by General Roy, in the Philosophical Transactions for 1790, being then unfinished. The radius of the arc is five feet, and the arc itself is of that extent to take in nine degrees on each side of the zenith. It is divided into degrees, and smaller divisions of 20′ each, which are numbered. Each of these last is again subdivided into four, of 5′ each. The micrometer, which moves the telescope and arc, is graduated to seconds, and one revolution moves the

arc

arc over 1′ 10″ 08‴, but the scale being large, a small fraction of a second can be easily defined. The construction, and improvements to the zenith sector, are so well known, that a minute description of it here would be unnecessary. It will therefore suffice to say, that as far as so delicate an instrument can be managed in a portable observatory, or travelling tent, which never can offer the advantages of a fixed, well contrived building, I have every reason to be satisfied with it.

The time I commenced observing at Paudree station was during the heavy part of the monsoon, which occasioned frequent interruptions: and although I had intended observing by at least three fixed stars, I only succeeded to my satisfaction in one, which was Aldebaran. With that star I had a fortunate succession for about sixteen nights; some few of these observations, being less favourable than the others, were rejected. Observations.

During the time I was at Trivandeporum, near Cuddalore, the weather was settled and serene, and the nights perfectly clear, so that I had an unlimited choice of stars, but having been successful with Aldebaran, I chose that star for determining the length of the arc.

As I consider the celestial arc more likely to be erroneous than any terrestrial measurement, I have thought it necessary to give some account of the manner of observing and of adjusting the instrument, for, after two years experience, I have found, that, notwithstanding the great powers of the zenith sector, extreme delicacy and attention are requisite, to render the observations satisfactory. The following method of adjustment I have always practised. After having brought the vertical axis nearly to its true position by the adjusting screw at the bottom, or so that the wire of the plummet would bisect the same dot when the telescope was moved to the opposite side, or half round on the axis, I then examined whether the dot at the centre of the horizontal axis was bisected, and whether the wire moved in the vertical plane clear of the axis; for unless it be perfectly free, all the observations will be false. When I had bisected the dot, I either took out the microscope and looked obliquely, or did the same by a magnifying glass, and by that Manner of observing, and adjusting the instrument.

that means I could discover the smallest parallax. If it admitted being brought nearer to the axis, it was done; but I found from experience, that it was more eligible to leave the wire at a sensible distance, than to bring it very near. Having satisfied myself in this particular, I examined with the microscope again in front, moved the wire freely in the vertical plane, and then bisected the dot. The telescope was then moved, so that the wire was brought over the dot zero on the arc, and the same precaution used with respect to the wire moving free of the arc; and here, as well as above, I found it best to allow a sensible distance between the wire and the arc.

Adjustment of the microscope. The microscope by which the upper dot in the horizontal axis is examined being fixed by the maker, the axis of vision is of course at right angles to the vertical plane, and will meet that plane in the centre of the axis; but the lower microscope is movable, and requires care to fix it so as to have the wire in the axis of vision, and be free from the effects of parallax; this I have done by moving it along the brass plate in front of the arc, till the wire appeared free from curvature, and then adjusted the dot. In these late observations, I have generally made the final adjustment by the light of a wax taper, for the wind being sometimes high and troublesome, I found there was much irregularity in the observations, until I adopted that method. I therefore closed the doors and windows of the observatory tent, so as to have a perfect stillness within. The distance of the wire from the axis and the arc is likewise better defined by a taper by noticing the shadow in moving the light to the right and left.

Fixing the instrument. In fixing the instrument for the star, great care was taken to have it placed in the meridian, which was done by a mark at near the distance of a mile (generally one of my small flags), the polar star having been previously observed by the large theodolite for this purpose. The telescope was then moved in the vertical, till the wire of the plummet was at the nearest division on either limb to the zenith distance of the star, which could always be nearly known. The micrometer, having been put to zero, was firmly screwed, and the dot on the limb carefully bisected, the instrument was turned

turned half round; the adjustment examined and corrected, if necessary. This being done, the degrees and minutes, &c., on the arc were noted down, as was also the particular division on the micrometer scale, at which the index stood, and the fractional part of a division in case there was any. In this state every thing remained to within fifteen or twenty minutes of the time the star was to pass, when I repaired to the tent, and again examined whether the wire bisected the dot; if it did not, the instrument was again adjusted to the same dot, and the horizontal axis also examined by the upper microscope, all this being done, the sector was placed in the meridian.

Observation of the star.

When the star entered the field of view, the micrometer was moved gently till the star was near the horizontal wire, but not bisected till it came near the vertical, that the micrometer might not be turned back, but continue moving in the same direction. This I did to avoid any false motion in the micrometer screw, and I was led to this precaution by the repeated experiments I had made in examining the divisions on the arc, for it sometimes happened after moving the arc over one of the divisions till the wire bisected the next dot; and then turning it back again, that the index of the micrometer was not at the same second, but had passed over it perhaps one, and sometimes two seconds; but by moving over the next five minutes in the same direction, the number of revolutions and seconds were always what they ought to be, to some very small fraction. This anomaly, however, only happened in some situations of the screw, and to avoid any errours arising therefrom, I adopted the above method.

Caution respecting the micrometer.

Zenith distance taken on both parts of the arc.

The zenith distance of the star being now had, on one part of the arc or limb, after the same process had been gone through the next night, with regard to the adjustment, the zenith distance was taken on the other part of the arc, by turning the instrument half round on its vertical axis. The mean of these two was therefore the true observed zenith distance, and half the difference was the errour of collimation. For applying these to the purpose in question, the mean of the zenith distances being corrected for refraction, the declination of the star for each of these nights was corrected for

 nutation,

nutation, aberration, &c., to the time of observation, and the mean of the two taken for determining the latitude.

In this manner has the whole series of observations been continued, by turning the sector half round every night, for the purpose of observing on opposite parts of the arc, and each compared with its preceding and succeeding one. In pursuing this method, it was unnecessary to notice the errour of collimation for any other purpose than as a test to the regularity of the observations; for until they became uniform, no notice was taken of the zenith distances, concluding that there had been some mismanagement, or some defect in the adjustment.

Degree of lat. between 12° and 13° N. 60495 fath. Deg. of long. in 12° 32′ 61061 fath.

After major Lambton had made out his account of the meridional arc, he completed the measurement of a degree perpendicular to the meridian in latitude 12° 32′ nearly, derived from a distance of upward of fifty-five miles, between Carangooly and Curnatighur, two stations nearly east and west from each other. The final results of his computations are, that the degree on the meridian in this latitude is 60495 fathoms nearly, and the degree perpendicular to it 61061 fathoms nearly.

VII.

An Account of some New Apples, which, with many others that have been long cultivated, were exhibited before the Horticultural Society, the 2d of December 1806. *By.* Mr. ARTHUR BIGGS, *F. H. S.*

The apple one of our best fruits.

OF all the different fruits, that our island affords, none can be brought to a higher degree of perfection, with so little care and trouble, especially in its southern counties, than the Apple. For a proof of this, I hope it will not be deemed presumptuous in me to refer to the catalogue below, every variety of which I had the honour of exhibiting to the Horticultual Society, at our meeting in December last. Having been flattered by the wishes of many gentlemen then present,

present, to give some account of such as are new, and by what culture they have been produced in such perfection, I cannot but attempt it, though very inadequate to the task, for almost every hour of my life has been employed in following the instructions of others, and when I have deviated from them, with a view to improvement, I have seldom been able to write down the result of my experiments with any satisfaction to myself.

Beside the sorts of apples lately exhibited, the garden of Isaac Swainson, esq., my indulgent master, contains a number of others, which are less valuable. When I mention that I am cutting these away as the better trees advance, and thinning the branches of the latter also as they require it, I perhaps tell all that is to be told upon the subject; for I have found nothing of more consequence to the health of the apple tree than plenty of light and air. The instructions of the late Mr. Philip Miller, on this head, are so pointed, and I see so many apple trees smothered either by their own branches or those of other trees, that I cannot do better than quote his words. After directing the standard trees to be planted at the distance of 40 feet every way, and the dwarfs at that of 20 feet, he says, "I am aware how many enemies I shall raise by retrenching the great demand, which must of necessity be made in the several nurseries of England, if this practice be adopted, but as I deliver my sentiments freely on every article, aiming at nothing more than the information of my readers, so I hope there will be found none of my profession of such mercenary tempers, as to condemn me for telling the truth, though it may not always agree with their interests."

Apple trees should not stand too close, nor their branches be too thick.

Large ones should be 40 feet, dwarfs 20 feet distant.

I feel no fear in referring to this great gardener's work, because all the principal nurserymen, who now supply the public in the vicinity of London, are men of too much liberality to recommend a less distance, than the above; and in the present opulent state of this country, the original price of the trees is comparatively so trifling, that if any one plants double the number which ought to remain, he will be repaid more than a hundred fold in the few years that the alternate trees are suffered to stand. This is a practice, therefore,

May be planted close, to be thinned as they grow.

 which

which I have not scrupled to recommend: but, after all, whether a gentleman plants many or few trees, his future success and gratification depend principally upon the judgment of his gardener, in choosing such trees in the nursery, as have been grafted from *bearing branches*; and if I thought myself authorized to give any hints to our nursery-men, it would be relative to the selection of their grafts and buds, not only in the apple tree, but every sort of fruit tree, about which they are in general too careless.

Should be grafted from bearing branches.

Grafts to be selected with care.

Apple trees raised with advantage from cuttings.

I must now observe that the apple tree will grow readily by cuttings, and that trees raised in this way, from healthy one year old branches, with blossom buds upon them, will continue to go on bearing the very finest possible fruit, in a small compass, for many years. Such trees are also peculiarly proper for forcing, by way of curiosity or luxury, and I believe that they are less liable to canker than when raised by grafting, though I am unable to assign any reason for it. I have more than once experienced this in the *golden pippin*, cuttings of which have remained seven years in perfect health, when grafts taken not only from the same tree, but from the very branch, part of which was divided into cuttings, cankered in two or three years. Accident, which brings to light so many useful things, first taught me this practice; some cuttings, that I had stuck into the ground for marks of annual flowers, having all made roots. The soil was loamy, and the summer proved so wet and cold, that many bunches of grapes in a large greenhouse, which I could not prevail upon the gentleman I then served to be at the expense of thinning with scissars, rotted when green.

These well adapted to forcing,

and continue in health longer than grafts.

This discovered by accident.

Effects of soil on the apple tree.

The soil at Twickenham is light, and inclined to sand rather than loam, in which the apple tree will ripen its fruit earlier and more completely than in a stiffer soil, but it will not last so long. Young seedling plants will also produce their blossoms and fruits in a shorter period in such soil. Our trees being originally placed too near each other, I have transplanted several into other quarters with very great success, even after they had attained a considerable magnitude. In doing this, I was careful to preserve every root possible both great and small, to have the ground where they were to be

May be transplanted large.

be planted ready open to receive them, so that their roots were only exposed to the air a few minutes, disposing their fibres as horizontally as possible, and not too deep. The months of September and October should be preferred for transplanting any large tree, watering it well if showers do not fall the same day: if the leaves are not pulled off, it will make fresh roots immediately, or at all events be more disposed to push them forth in spring. I constantly tread the ground exceeding firmly with my feet, in separate layers of about an inch, so as to render staking unnecessary, a practice which if performed so as to have any real effect is very expensive, but which too frequently does more mischief than good.

Treading the ground well on the roots preferable to staking.

Of the varieties of the apple cultivated in Mr. Swainson's garden, which ripen early, I can especially recommend, the summer pippin, Devonshire quarrington, summer traveller, bland rose, summer pearmain, red colville, marigold, Kirk's incomparable, Evan's valuable, nonsuch.

Early ripening apples.

Of the autumn and winter varieties, perhaps all those which follow are valuable, especially such as are marked with a star, and those marked with a cross are new.

Autumnal and winter varieties.

*Norfolk storer, *Norfolk beaufin, Norfolk paradise, Holland pippin, embroidered pippin, striped Holland pippin, *lemon pippin: as this variety is beginning to canker in many gardens, there is no doubt that it is old, and has been introduced from the continent, probably Normandy: for a gentleman who was at Rouen, during the last short peace, saw it there in abundance.

*Ribston pippin, New Town pippin, *golden pippin, Marmail pippin, French pippin, Kirton pippin; Wyken pippin, Fern's pippin, London pippin, *Kentish pippin, New Town late pippin, mathematic pippin, †William's pippin, Whitmore's pippin, New York pippin, raspberry pippin, cat's head, *king of pippins, nonpareil codling, Cowring's queening, *flower of Kent, Selleswood's reinette, *Holland berry, golden mundi, margill, nutmeg apple, royal russet, golden russet, Pile's russet, Clifton crab, *Minchin crab, French crab, Herefordshire pearmain, Loan's pearmain, Holt's pearmain, Kentish reinette, lady's thigh, pigeon's egg, Tolworth court, spice apple,

apple, quince apple, hall door, *transparent pippin, *golden reinette, golden royal, †Bigg's nonsuch, †flat green, †false beaufin, summer breeding, cœur pendu, †Minier's dumpling, †Padley's pippin, †oval apple, †green pyramid.

Description of the best.

To give a complete history of each of the new apples above mentioned is out of my power: they have all been raised by other gardeners, from whom we may rather expect it: in the mean while, however, the following descriptions will perhaps suffice to make those which appear to me the best, more known.

William's pippin.

William's pippin. Size, from 2 inches to 2½ inches long. Colour, pale yellow, with a little red on the sunny side, and here and there a spot. Shape, somewhat conical, scarcely longer than broad, deeply umbilicated at the stalk, which is short, hollow at the top; the leaflets of the calyx, though black and dry, still remaining more perfect than in many. Flesh, pale, yellow, soft, excellent to eat ripe from the tree, baking and roasting well, till Christmas.

Padley's pippin.

Padley's pippin. Size, from 2 to 3 inches in length. Colour, rich yellow, generally very finely laced all over with a pale rough starry bark, if I may use the term. Shape, oval, about the stalk flat, or often a little prominent on one side, not much depressed about the calyx, which is more obliterated than in many others, perhaps from this circumstance. Flesh, firm and juicy, of a rich perfumed and poignant flavour, in high perfection all December and January. I am inclined to think this the very best of our new apples.

Bigg's nonsuch.

Bigg's nonsuch. Size, from 2 to 3 inches in length. Colour, deep yellow, striped and variegated with red on the sunny side. Shape, and general appearance, somewhat like the nonsuch, but broader at the base, moderately depressed about the foot-stalk, very hollow at the top, where the leaves of the calyx remain long and rolled back. Flesh, pale, yellow, soft, and excellent to eat ripe from the tree; roasts and bakes well till Christmas.

Minier's dumpling.

Minier's dumpling. Size, from 3 to 3 inches and a half in breadth, but not so long. Colour, deep green, and very dark red next to the sun; which, together with its spherical shape, more contracted at the top, and swelled into a few imperfect angles,

angles, give it some appearance of the Norfolk storer, but there are darker green lines on the north side which distinguish it from all the apples I know. It is depressed about the stalk, which is long and stout enough for so large an apple. The calyx is nearly obliterated by the time the fruit is ripe, which is not till Christmas, or after. It is most valuable for boiling or baking till April, and even to eat at the end of the season; its flesh firm, high flavoured and juicy.

VIII.

On the Cultivation of the Polianthes Tuberosa or Tuberose. By RICHARD ANTHONY SALISBURY, *Esq. F.R.S. &c**.

THE charms of Horticulture, in every civilized nation, have been acknowledged by men of all ranks, from the highest prince down to the lowest cottager. While the graver duty of the historian has been simply to commemorate the calm and innocent delights which it affords, the holy mythologist has exalted it as the sole employment of our first parents in Paradise; and poets have embellished their most enchanting verses with its productions: so that to offer a long and laboured panegyric upon any single branch of it, to a Society instituted for the express purpose of encouraging them all, would, in the emphatic language of an old writer, be like vainly attempting to paint the lily, add a perfume to the violet, or gild refined gold. The field before us, moreover, is no less extensive than that of the whole globe, which is in fact one immense garden, covered with vegetables common to every animal that exists; but Providence has in infinite wisdom allotted to man the proud preeminence over all; his wants, if he is not indolent, being invariably first supplied. In those earlier stages of society, however, when the ground was first cultivated, it must have been

Art of gardening.

* Abridged from the Trans. of the Horticultural Soc. vol. I, p. 41.

been inconceivably difficult to exclude various animals, both carnivorous and herbivorous, from the immediate precincts of human habitations; driven as they now are from every populous country, we can form but a very imperfect idea of their tremendous power and strength in warmer climates, while thinly inhabited. Hence the progress, even of Agriculture, was in all probability for a long period slow and interrupted: years and years must have elapsed, before her younger and more delicate sister, Horticulture, ventured to appear; though, to plant a clump of *bananas*, which would give immediate shade, and to perfume the surrounding air with the fragrance of an *orange* grove, independent of the fruit these two vegetables afford, must have been natural, one would think, to many a savage of finer feelings, the moment his residence became fixed.

subsequent to agriculture.

Tuberose recommended as profitable.

To leave the language of fancy for that of fact, I know no ornamental plant, which seems to me more deserving of cultivation in the warmer soils of this kingdom, or that would repay the labour attending it with greater profits, than the *tuberose*.

First account of this flower.

The first account that I find of the *tuberose*, is in l'Ecluse's History of Plants, where it appears that on the 1st of December 1594, he received a specimen of it, in very bad condition, from Bernard Paludanus, a physician at Rome, to whom it was sent by the celebrated Simon de Tovar, of Seville. It certainly had not then been many years in Europe, and Linné, in his Hortus Cliffortianus, on this head refers us to Plumier's Genera Plantarum, p. 35, who says it was first brought by Father Minuti, from the East Indies, into the senator Peiresc's garden at Boisgencier, near Toulon. It is much more probable, however, that it was introduced at an earlier period, and from America, for no author describes it as wild in the East Indies. Loureiro only found it cultivated in the gardens of Cochin China, and Rumph says it was unknown in the Island of Amboyna, till the Dutch carried it there from Batavia, in 1674. On the contrary, Kamel informs us, that it was brought to the Island of Luzone, by the Spaniards, from Mexico; and Parkinson, in 1656, tells us, that the plants, which he describes as two species, "both grow naturally in the West Indies, whence being

Supposed from the East Indies.

More probably from America.

Natives of the

first

first brought into Spain, they have from thence been dispersed unto divers lovers of plants." The senator Peiresc, as may be learnt from Gassendi, was only fourteen years old in 1594, when Simon de Tovar had already cultivated it at Seville, and according to Redoutè, it was not planted in his garden at Boisgencier, by Father Minuti, till 1652, whom that author makes to have brought it from Persia. I only infer, however, that he travelled from Hindostan over land. Redoutè moreover asserts, that the authors of the Flora Peruviana found it wild in America, but in the work itself they say, cultivated in gardens. Hernandez' evidence, however, I think, takes away all doubt about the matter: he says, " provenit in frigidis et temperàtis regionibus, veteri incognita mundo," and as the *agave*, to which the *tuberose* is more immediately allied, is also a native of Mexico, I am fully of opinion that it is indigenous there. West Indies.

L'Ecluse an accurate observer.

The description given by the venerable l'Ecluse, of his specimen, half dried and battered by the journey, with only the lowest flower of the spike expanded, affords a memorable instance of his accuracy and discernment. The size of the stem, insertion and figure of the leaves, and their hempy texture, are particularly noticed; the shape of the corolla, with its general similarity to that of the *Asiatic hyacinth*, but in consistence rather to that of the *orange*, is next remarked; and having no knowledge of the root to guide his judgment, but what he derived from Simon de Tovar's appellation of *Bulbus Indicus florem album proferens hyacinthi Orientalis æmulum*, he guesses it may possibly belong to the same genus with the *bulbus eriophorus*, or *Peruvian hyacinth*, though not without some doubts raised by its stem being covered with leaves, and its tubular corolla. Two years afterward, these doubts were corroborated by his receiving roots both from Simon de Tovar, and the Comte d'Aremberg, which by August were full of leaves; and I think it worth noticing, that his figure of the plant appears evidently to have been made up from the original specimen sent by Bernard Paludanus, and one of those growing roots, which he expressly mentions did not flower: he concludes with observing,

serving, that if it is still to remain in the genus, it may be called *hyacinthus Indicus tuberosâ radice.*

Origin of the name.

From this latin phrase, no doubt, our silly appellation of *tuberose*, and the more accurate French name, *tubereuse*, originated; but in the East Indies it is distinguished by the poetical title of *sandal malum*, or *intriguer of the night*; in Spain, where at the period of this plant's being discovered it was the fashion to give both places and things religious names, it is called *vara de S. Josef.*

East Indian name.

Figures of it. Vallèt. Swertius.

Soon after l'Ecluse's figure, an excellent one by Vallèt the embroiderer came out at Paris in 1608, and both these were copied and published as different species, by Swertius, in his Florilegium. An original figure, which has great merit for that day, though not equal to Vallèt's, next appeared in the Theatrum Floræ, my edition of which, I believe the earliest, bears the date of 1622; it shows many roots flowering in one pot. From Ferrarius's pompous book on the culture of flowers, we learn it was still regarded as a rarity in the Barberini gardens, at Rome, in 1633, but that it increased abundantly, and was taken out of the ground every year in March, to separate the offsets. Our countryman Parkinson, more than half a century after its being first described by l'Ecluse, is the next author who treats of this plant; but valuable as many of his quaint observations still are to the horticulturist, his account of the *tuberose* does him little credit; he makes two species of it, saying, he thinks l'Ecluse never saw the first, though he owns "some do doubt that they are not two plants several as of greater and lesser, but that the greatness is caused by the fertility of the soil;" his figures are wretchedly copied from Swertius, and by his calling it the *Indian knobbed jacinth*, it appears not to have been known here then by its modern name. Gaspar Bauhin, with his usual carelessness, also takes it up as two species from Swertius, and even the learned Ray seems to have known as little about it in 1693, adding, however, to his second species, the title of *tuberose.*

Ferrarius.

Parkinson.

Bauhin.

Ray.

Miller.

I meet with nothing more of any consequence respecting it, till Philip Miller, the pride of every British gardener, published the first edition of his Dictionary in 1731. He makes

makes it a distinct genus from *Hyacinthus*, and describes the variety with double flowers, now so common, but then only to be seen in Monsieur de la Court's garden, near Leyden, whose memory is most justly consigned to infamy by our author, for destroying many hundreds of the roots, rather than parting with a single one to any other person; an instance of narrowness of mind and illnature, he adds, too common among the lovers of gardening. I trust no one who belongs to this Society will ever deserve a similar reproach. At this period we find the roots were annually imported into England, along with *orange* trees and *myrtles* from Genoa, and to the directions there given for blowing them, so as to have a succession of flowers from June till October, nothing can be added.

Instance of selfish illnature in a florist.

Imported with orange trees & myrtles from Genoa.

Though our gardens now are enriched with a profusion of other fragrant and beautiful flowers, the *tuberose* still continues to maintain its superiority, and we receive roots, especially of the double variety, from the warmer provinces of North America, as well as Italy. There is no necessity, however, to be indebted to foreign countries for this supply, as I can speak from experience, having cultivated it in the open air for many years at Chapel Allerton, notwithstanding the average temperature of that hill from the month of April to October is far less than in the adjacent valley. If a sufficient degree of heat in summer can only be obtained to bring the leaves out to their full magnitude, that of the roots follows of course, and very little more care than what is bestowed upon the *artichoke*, will preserve them from the severest frosts.

Still much prized.

May be cultivated at home.

For this purpose, select a piece of ground that is perfectly drained, under a south wall; or, if this cannot be spared, defend it on the north by a reed hedge. The size of the bed must be proportioned to the number of roots you want, for the same tuber never blows a second time, but only the lateral ones, which are produced in great abundance round it: as they are to be planted at five inches distance from each other, a bed nine feet long, by three feet wide, will hold 144 roots.

Method of culture.

The soil, in which I have found them succeed best, is light sandy

Soil.

sandy earth, mixed with a third part of very rotten cow dung: the earth should be taken about seven or eight inches deep, along with the green turf, chopping it very small with the spade, and turning it once a month for a year before it is used; if the earth is not very light, add a quantity of sea sand, or fine shelly gravel. If you are obliged to use this compost sooner, pass it through a wide screen, casting out nothing but any large stones.

Preparation of the bed.

About the middle of April prepare the bed as follows: first, take out all the old earth, to the depth of two feet and a half, or three feet, filling it nearly to the top with fresh stable dung, that has been cast into a heap to heat a fortnight before: lay the dung evenly in the trench, treading each layer very firmly down with a board, under your feet, and reserving the smallest and shortest for the last: upon this lay eighteen inches in depth of the compost, sloping it well towards the south, not only for the benefit of the sun, but to throw off violent rains.

Planting.

In a day or two after, plant your roots at five inches distance from each other, observing to place them alternately in the rows, and that the crown or upper part of the tuber is only just covered with earth. These should be the offsets of such as after flowering the preceding year have been preserved from frost through the winter in sand, as well as the strongest remaining upon any fresh imported ones. Till you obtain a sufficient stock, even the weakest may be planted, but as a great number are annually produced by every root, in time those which are large enough to flower the following year need only be selected.

Management after planted.

Cover the bed at night, especially if frosty, with a double mat, till the leaves appear, but give little or no water, protecting it carefully from heavy rains. When the leaves are about an inch long, add a little fresh compost to the surface, filling up any inequalities, and removing all weeds. If the season prove dry, it will now require watering, and towards the end of June and in July, when the leaves are in full vigour, very copiously; but this must depend upon the weather. From this period till the beginning of winter, nothing more is necessary than to weed the bed, and protect it from the autumnal rains: this may be

be done by sloping the ground more up to it, or if you have a cucumber frame not in use, it may be employed for this purpose, taking care to sink the front so low as to admit all the sun possible. About the first week in December, take the advantage of a dry day, and after clearing away all the decayed leaves, thatch the bed all over, and at the sides, a foot thick with dry straw, sloping it well to throw off the wet.

About the middle of February, if not prevented by severe frost, take up all the roots, preserving their fibres, and pack them in very dry sand, in cellars where the cold cannot penetrate till April, when they must be replanted as before, shortening their fibres more or less, as you find them decayed. If the climate was even milder than ours, I should recommend the roots to be taken out of the ground, and preserved in dry sand, for it throws them into a complete state of rest, and disposes them to form their flower stems earlier. Many offsets will by this time have made their appearance round each root, all of which, except two or three at most of the strongest, should be cut entirely out, and this operation must be in some degree repeated after they are planted and growing, as fresh offsets are produced, for, if permitted to remain, they will rob the other buds of sufficient nourishment.

Roots to be taken up in February, and replanted in April.

This second year some of the largest roots will probably flower: if they send up their stems early it will only be necessary to stick them carefully, when about a foot and a half high, and leave them to blossom in the open air; but when they appear later than July, they should either be removed into pots, with a trowel, preserving all the fibres possible, and placed in a stove, or if you have not that convenience, cut out the flower stem, with all the central leaves, as soon as it is discovered, which will strengthen the offsets. In the succeeding winter thatch the bed, taking up the roots in February, as before, most of which will now be strong enough to flower, and may be selected for sale: such roots, if wanted for early forcing, will have a decided advantage over imported ones, for, as their fibres will not be entirely decayed, they will

Management in the second year.

will push immediately upon being removed into brisk heat, and may be brought to flower as early as May.

Estimate of expense and profit.

According to the abovementioned distances, half a quarter of an acre would contain 15,125 roots, leaving nearly as much space for the alleys as the beds, which, at 3d. each, amounts to the sum of £189 1s. 6*d.* and as when a sufficient stock of offsets to select the largest was obtained, the annual return of blowing roots may be estimated at half the number planted, the profits of a bed of *tuberoses*, after deducting every expense of rent, dung, and labour, would be considerable, even if it were necessary to cover it in autumn and winter with three light frames.

Places favourable for it.

There are many places in our Island where I should imagine this plant might be cultivated with still less care and attention, especially in the southern counties near the sea; in the vicinity of London. Ham Common, Sunbury, and Walton upon Thames; in the Isle of Wight; about Southampton; below Exeter; Bath and King's Weston; in South Wales: and the theory which I would recommend any intelligent gardener to adopt in its general management is, to keep the roots growing as vigorously as possible from May to October, but in a state of complete rest and drought for the remainder of the year.

General remarks.

IX.

Geological Remarks on a calcareous Mountain near Chessy, in the Department of the Rhone: by Mr. L. F. Lemaitre, *Inspector General of Gunpowder and Saltpetre.**

All natural facts worthy notice to the geologist.

I Do not think there exists a natural fact, or an obfervation however slight, that does not merit the attention of the geologist, who should study incessantly the voluminous book that Nature has laid open before his eyes. Certain pages of this book it is true may appear but little interesting; yet he should not pass over a single one if possible, would he attain an accurate knowledge of the interesting history of our globe, and the wonderful revolutions it has undergone. A mountain,

* Journal des Mines, No. 106, p. 307.

a mine,

a mine, a quarry, an earthslip, are so many pictures, in which geologists may discern this history.

From the same considerations I am induced to think, that they will be gratified by the account and sketch I here present them, which appear to me to exhibit something singular, if not problematic, which it is for them to solve. See Pl. II, fig. 1.

The schistose vale, in which the village of Chessy, near Lyons, is built, is bounded on the north-east by a chain of mountains of no great height, which appears to run south-east and north-west, and in which a well known mine of yellow sulphuret of copper is wrought. On the opposite side of the vale is a chain of mountains of two or three hundred yards high, nearly parallel to the former chain, but not stretching so far to the north-west, and cut about three quarters of a mile from Chessy by another vale, meeting the first almost at a right angle. Vale near Lyons.

The last mentioned chain is calcareous from its summit about two thirds of its height. Its base appeared to me to be composed of a schistose rock, similar to that which composes probably the first chain and the whole of the intermediate valley, since the vein of copper, which has been wrought to the depth of upward of a hundred and sixty yards, is enchased in this rock. Calcareous mountains on a base of schist. Copper mine.

The extremity of the high calcareous chain, at the kind of promontory it forms where the two vales meet in an angle, exhibits at its summit a large quarry of calcareous stone, which is used for building in the adjoining country. A perpendicular section eighty or ninety feet high, made in a direction nearly east and west, exhibits a series of strata from eight to fifteen inches thick, not arranged horizontally, but with different degrees of inclination to the horizon, and crossing one another in various directions, as seen in Pl. II, fig. 1, which is from a drawing taken on the spot. The value of the angle I have inserted at the different arrangements of the strata is only estimated by sight, as I was unable to measure it, on account of the steepness of the place. It is according to the decimal division. All the strata in each arrangement are parallel, except those marked A & B, which grow wider, the first as it descends, the second as it follows Limestone quarry at the top. Strata variously inclined. Two strata widen in their course.

follows the curvature of the strata on which it rests. Each of these is very distinctly separated from that which immediately follows it, or from the head or base of those it covers, or which abut against it by a kind of saalbande of the same nature as the strata, but of another colour.

Stone coloured by iron, and contains a few shells.

The stone of this quarry has a pretty fine grain, is rendered yellowish by oxide of iron, and contains a few shells. There are found in it small bivalves of the chama kind, crowstones or gryphites, and a few belemnites. The shelly and coarse strata serve for building, the fine and hard strata are used for entablatures and other ornamental parts.

Mine counsellor Gillet l'Aumont has given such a satisfactory explanation of the angles and tortuous bendings of certain veins of coal, and other alluvial strata, such as those of bog iron ore near Sarre-Libre, that he seems to have caught Nature in the very fact. Whether his ingenious hypothesis will account for the arrangement of the strata in the quarry at Chessy, I must leave to his consideration.

X.

*Remarks on a singular Arrangement of Strata observed in the Chain of Jura, in the Department of Doubs: by the Same**.

Of what use is the observation of Nature?

OF what consequence is it, some will say, whether the constituent parts of our globe be arranged in this way or that? What signify to us the causes of the regularity or disorder they may exhibit, if the order of Nature as a whole be not disturbed; if every thing in the universe be as it ought to be?

It may be abused,

No doubt the abuse of observation, for every thing has its abuse; no doubt the desire of explaining every thing, not excepting what exceeds the limits of our narrow comprehension; have led natural philosophers into useless researches, and into idle explanations, that frequently betray more vanity, than desire of being useful: but I conceive there are

But is certainly beneficial.

facts in geology, which it is advantageous, I will not say in

* Journal des Mines, No. 106, p. 310.

all

all cases to explain, but at least to observe accurately, and to make known, because they are of importance to an art of essential utility, an art founded on observation, that of the miner.

Lead to a knowledge of mines.

The course of the strata of combustible minerals and metallic veins, their various directions and inclinations, their bendings, turns, faults, disappearance, and change of position, their impoverishment, &c.; all these different states, all these modifications, appear to depend on the arrangement of the strata of our globe, and the concussions they may have experienced at different periods, whatever the causes may have been. Perhaps therefore it is not useless, to make known any singularities of this kind, hitherto little observed, that may offer themselves.

Thence useful to the miner.

Since it is from the bosom of the earth we derive the materials of our most useful arts, minerals, to study its internal constitution is of importance, as this would frequently lead to the solution of the difficulties that present themselves to the miner, or at least diminish their number. His progress therefore, being less uncertain, would be much less expensive.

The author's claim to confidence.

If my reasoning be just, I shall bring forward with more confidence a few observations, that appear to me to claim the attention of geologists, from the singularity of the facts. They may confide in what I have the honour to lay before them, since, as I am not sufficiently initiated into natural history to form systems, I have always confined myself to observation, and to observe long before I copy; and in the present instance I have the confirmation of several fellow travellers, among others the senator Aboville, whose accuracy of observation is well known.

Jura.

The table land of Jura, on which stands the city of Pontarlier, is furrowed by a few valleys, more or less close. One of the most interesting is that of la Loue, on account of its wild and picturesque scenery, and the various works constructed on the banks of the river. It is rendered particularly remarkable by the source of the river itself. I do not think the reader will be displeased with my saying something here of this wonder of Jura, as it may be styled, which deserves

Vale of la Loue.

serves on many accounts to be visited both by the naturalist and the lover of the arts.

The vale described.

The valley of la Loue begins above the village of Monthier-Haute-Pierre, between Pontarlier and Ornans, in the subprefecture of Pontarlier, in the department of Doubs. This valley, very narrow at its origin, and very deep, and almost perpendicular throughout, exhibits in this part the appearance of a vast well, opened on one side to let the water flow out. Its sides are composed of compact, gray, calcareous rocks, veined with white carbonate of lime in a state of confused crystallization. At the foot of these rocks, but nine or ten yards above the bottom of the valley, a dark cavern, the depth of which is unknown, and the mouth of which is about seventy yards wide and thirty-five yards high, pours out with great noise a very copious torrent of clear water, that tumbles foaming among the rocks, which it has torn off and driven before it. The depth of the valley, the beetling cliffs that form it, the aspect of the cavern, the roar of the torrent rushing out of it, the mist it throws up, the gloom that reigns in this savage place, the bottom of which has never been illumined by the rays of the Sun confined to the tops of the rocks, all conspire to give an idea of the chaotic disorder, that prevailed before human industry had laid earth, water, air, and fire under contribution for the benefit of the arts.

Cavern, from which issues a river.

Number of manufactories established on it.

By regulating the course of these waters, or of part of them; gaining by the explosion of gunpowder a few yards of surface from the adjacent rocks; and by suspending erections over the torrent itself; a number of different manufactories have been established at the foot of this precipice, forming a complete contrast between art and nature. The Loue, after it issues from the cavern, is divided and turned in numberless directions to set in motion eight or ten flour mills, oil mills, mills for bruising hemp, forge bellows, large and small hammers, flatting mills, cylinders for cutting iron into bars, and saw-mills. These wonders, which are daily increasing by the addition of fresh structures, are owing to the industry and activity of Mr. Besson, administrator of saltworks. I had forgotten to mention, that you get to the bottom of this enchanted precipice by a flight of steps, the windings

windings of which, concealing it from your view till you reach it, render the picture more magical, and the surprise the greater.

It was in the cliffs forming the walls of this narrow basin, that I had an opportunity of noticing some singular arrangements of the strata composing it. Every thing exhibits traces of the derangement I had observed in many other parts of Jura, but here they appeared to me larger and more varied than any where else. I cannot give a better idea of them than by the drawings I made on the spot, engravings from which are annexed. See Pl. II, figs. 2 and 3. Strata distorted

It is to be remembered, that fig. 2 represents the face of the rock to the right of the cavern, the entrance of which commences at a very little distance from the natural vault A; so that the cavern has been opened through the strata B, B, and those resting upon and parallel to them, which dip toward the centre of the mountain at an angle of about thirty degrees. This fact will give an idea of the effect and of the time required for the water, or the acting power whatever it was, thus to force its way through immense strata of a hard and compact rock. Cavern.

The strata A, C, &c. exhibit a semicircular arch; those marked D, D, an elliptical arch. Both appear to rest on the strata E, E, which are no doubt produced to the left under an angle nearly similar to that of the strata B, B; and probably receive the extremity A, C, of the smaller arches. Natural arches.

All the little veins or laminæ, that compose each stratum, have regularly undergone the same curvature; so that the arcades and all the curved parts of the strata of this mountain exhibit the appearance of a book bent in different directions. Laminæ of the strata regularly curved.

I could not get at the knowledge of the arrangement of the part below, F, F, which forms the floor of a sort of yard belonging to the manufactory. Mr. Besson is making an excavation under the natural arch D, D, for the purpose of a storeroom or workshop. Part beneath not visible.

The existence of the portions or rudiments of strata, G, G, G, H, H, H, will perhaps appear singular, but it is not the less real, and is very distinct. If I might be permitted to hazard an opinion respecting them, it would be, Depositions in the angles of the arches.

 that

that, from these depositions in the angles of the arches we must infer, that the arches were in being before these depositions were formed.

Another part of the mountain. Natural arch. Fig. 3 represents another face or precipice of the same mountain contiguous to that represented in fig. 2; so that it forms one contiguous mass, but with an obtuse saliant angle at *a b*. In this part too there is an arch I, of larger dimensions than either of the others, and much flatter. It seems to form a support for the upper strata, L, L, L, which rest on its extrados.

Contorted strata. The singular contortion of the strata K, K, is represented exactly as it is in nature.

Strata intersecting. The strata L, L, L, M, M, M, and N, N, cross and mutually intersect each other, without losing any of their regularity in this part. Though I observed them from a distance, their general and reciprocal arrangement is too conspicuous, for me and my fellow travellers to have been deceived.

Similar appearances in other parts of the mountains; In thus observing the two sides of the mountain analogous dispositions of the strata are observable. I give here the most striking, but all of them merit the attention of the geologist.

and elsewhere. The mountains of Jura however are not the only ones, in which I have noticed phenomena analogous to those I have described. I have had opportunities of observing such in the calcareous mountains of the Lyonnese, in that which overlooks the village of Chessy, seven miles north of Lyons, toward the west, in a quarry on its summit, and of which I lately sent an account and drawing to the council of mines. See the preceding article, and Pl. II, fig. 1.

In coal and other mines we every where find examples of great disturbances happening to the surface of our globe, disturbances that must have occured at periods very remote from each other, and that excite our astonishment. Ages to man are but moments to nature.

XI.

Nicholson's Philos. Journ. Vol.XX. Pl.II. p.64.

Strata in a calcareous Mountain near Chefsy.

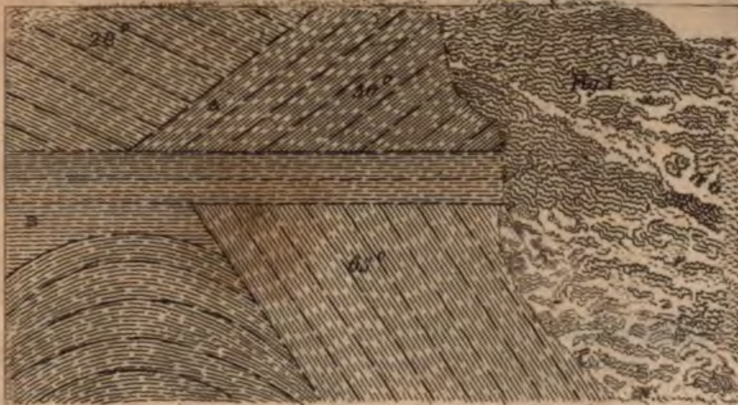
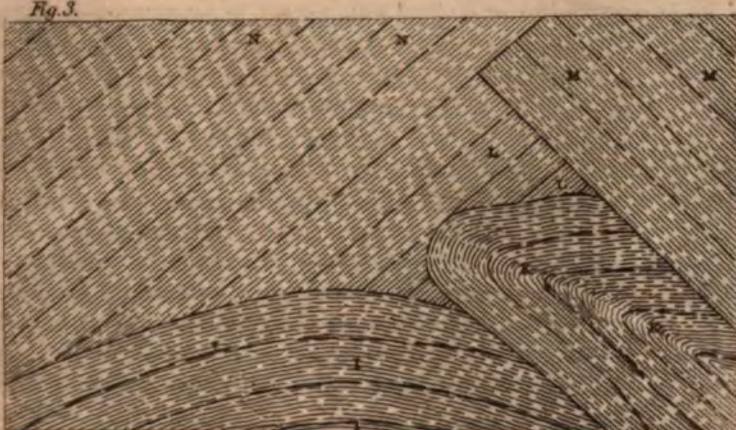


Fig.2. Strata in the Department of Doubs.

Fig.3.



XI.

An Inquiry into the Causes of the Decay of Wood, and the Means of preventing it. By C. H. PARRY, *M. D.*

(Continued from Vol. XIX, p. 338.*)*

WHEN wood decays under cover, that condition is usually called the dry-rot. Let us examine the circumstances in which this change takes place. Dry-rot.

It affects the interior doors, shelves, laths which subdivide the layers of wine, and all other wood work in certain cellars; beams and rafters which support the roofs of close passages; joists laying on or near the earth; the wainscoting of large rooms, little inhabited, in old and especially single houses; and wood in various other situations of a similar kind, which need not be particularized. In some of these cases, while one sample or portion of wood shall suffer the dry-rot, another specimen or portion shall remain unchanged. In other instances, wood of various kinds and qualities has been successively employed, and all has alike suffered. During the stages of change, a crop of mucor or mould, and very frequently of fungi, has sprung from the porous mass; and the decay is always attended with a wide-spreading exhalation, the odour of which cannot well be described, but which is sufficiently known. Places where it commonly occurs. Attended with mucor, or fungi, and a peculiar smell.

What then are the causes of this destruction; Precisely the same as those which I have before described; though their action is differently modified, and less obvious to gross observation. The decay is produced by the putrefactive fermentation of the component parts of the wood, in connection with moisture, without which, as I have before stated, wood cannot putrefy. Cause.

Common air is not only capable of mixing with a considerable quantity of water in form of vapour, but during every state of our atmosphere is always much loaded with it. Water becomes vapour in consequence of being united with a certain proportion of that substance which is called heat. Air loaded with water,

which is deposited on any thing colder than the air.

heat. If a sufficiently cold substance comes into contact with vapour, the superabundant heat, which was necessary to its existence in that form, passes into that cold substance, and the vapour is then immediately condensed or changed into water. Thus if in the hottest day in summer, when the vapour in our breath is totally invisible, we breathe on a looking-glass or plate of polished metal, which is colder than our breath, the surface is immediately dimmed; and if we continue to breathe on it, small drops of liquid appear, which gradually become larger and larger, and many of them at length uniting, run down the surface in a stream. The same thing takes place on the outside of a glass of water drawn in summer from a deep well, and of a bottle brought up into a warm room out of a cool cellar; and on the inside of our windows in frosty weather. On the other hand, we could not dim with our breath a plate of metal or glass of 100 degrees of heat, which is greater than that of our breath, and no mist is observable on the inside of our windows during the heat of a summer's day; nor is there any condensation of moisture on the outside of a glass of cold water fresh drawn from the well, or of a bottle out of a cellar, when either is brought into the open frosty air.

Many circumstances explained thus.

These circumstances will explain many appearances, by which, for want of due examination, we are often greatly puzzled. We are frequently mortified by seeing in our houses, especially in the country, the walls become stained, or the paper separated and hanging down, and often perishing; and as this usually happens on the side or corner which is most exposed to the weather, we conclude that the damp comes through the wall, and tax our faculties to the utmost, in order to prevent this penetration. The measures which we employ sometimes succeed. But it often happens, that casing, and plastering, and painting the devoted angle fails; and then, as the last resource, we take off the paper and attach it to canvass at the distance of one or more inches from the wall, and thus, for the present at least, effect the desired purpose. Now in this case it is just as absurd to suppose, that the wet comes through the wall, as that it comes through the glass window in a frosty day, or the glass or bottle from the well or cellar. The fact is, that in an exposed

Dampness of certain walls.

The wet does not come through the wall,

posed

posed house, and more especially on the most exposed corner of a room seldom warmed by fire, the inner surface of the wall, by the continuance of frost, is become of a very low temperature, like the air within the room itself. So long as this state of equal temperature between the wall and internal air continues, or if the wall is warmer than that air, it is obvious that the vapour which is mixed with the air cannot part with any heat to the wall, and therefore will not undergo condensation; just as no dampness appears on our windows during a hot day in summer. But if a thaw comes on, and the air becomes warmer than the wall, which, from its capacity of easily shifting place, it will readily do, then the vapour, which is mixed with it, parts with its superabundant heat to the colder wall, and appears on it in moisture or drops, or pours down it in streams; just as happens to the cold bottle brought into the warm dining-room.

but is deposited from the air in the room.

This change is the greater, the more completely the materials of the wall fit it for carrying the heat out of the vapour, or, in philosophical language, the better they conduct heat. Hence a wall painted in oil condenses vapour, or runs with water, sooner than one, which, being unpainted, is more porous; for which reason, in cities, we first perceive dampness and drops or streamlets of water on the oil-painted party walls which bound our staircases, and which are, therefore, absurdly said to sweat, though these walls have no communication with the outward air, and, from their varnished covering, cannot admit of the passage of moisture or perspiration through their pores.

A wall painted with oil soonest wet.

In this case the remedy is obvious, and by its success shows the nature of the evil. Prevent your walls from ever becoming colder than the warmest external air of winter, and you will never have this appearance of damp on their inner surfaces.

Principle of prevention.

This may be done, first, by constructing the walls of such a degree of thickness, or with such a disposition or quality of materials, that they shall not, in the usual way, be greatly cooled throughout their whole substance by any temperature of the outward air. With this view, I think that in all single houses, which are not warmed by neighbouring fires, and more especially in situations exposed to high winds, and therefore

Method of applying it.

Detached houses require thicker or double walls.

therefore to great evaporation from the external surface, and consequent abstraction of heat, the walls should always be double, having on the inside a thin layer of brick, with an interval of one or two inches from the outer and thicker layer of brick or stone, to which it must be united by proper binders. The porous structure of the bricks, added to the impermeableness of the intermediate stratum of air, would so ill conduct heat, that such walls would necessarily tend to keep a house dry and warm in the winter, as well as cool in the summer. This end would be still further promoted by filling the interval between the two layers with dry sand, fresh sifted coal-ashes, or powdered charcoal. In fact, when the common external means before described have succeeded in curing dampness, it has been either by affording a varnish, which has diminished evaporation by preventing absorption, or by increasing the space or changing the quality of the materials of the wall through which the heat was to pass, so as in either of these cases to retain it more forcibly: And when the dampness has been remedied by removing the paper to some distance from the wall by means of strained canvas, that effect has been produced by rendering the paper a worse conductor of heat; and therefore indisposing it to condense the vapour in the room so readily as when it was in contact with the colder wall.

How the common methods sometimes succeed.

Not possible to keep out the cold.

It has been suggested, that it would be possible to keep out cold, or, in more accurate language, prevent the egress of heat from the inside of a room, and therefore from the walls surrounding it, by shutting it closely up, and preventing any admission of the cold external air. This has arisen from the supposition that air is not a good conductor or transmitter of heat through its substance or pores, but that it merely carries it by changing place with some other portion which was less charged with it. If there were no other mode of abstracting the heat from the walls of a room, and if it were possible wholly to prevent any change of its air, this theory might perhaps apply. But it is not possible to prevent some exchange of this kind through the atmosphere of any habitable chamber; and it is evident from the moisture being most abundantly, or perhaps solely, deposited on the inside of that part of the wall which is most exposed to

Not wholly,

the

the external cold, that the chief or common mode in which the wall is cooled is not by the access of the cold air into the room, but by the passage of heat from the wall itself into the cold air without. We may however so far avail ourselves of this principle, as to exclude as much cold air as we can, by shutting up the windows and chimnies of uninhabited rooms during the severity of frost. but in part.

It may farther be suggested, that as, during a thaw, the air, being warmer than in frost, has a greater quantity of water in form of vapour mixed with it, shutting up a room on such occasions may, by retarding the admission of warmer air so charged with vapour, allow time for the walls to acquire an equable temperature through their substance from without, so as to anticipate any condensation on their surface which might occur from the free admission of the external air. To this I only answer, as before, that rooms according to the common construction cannot be excluded from communication with the external air; and that, in fact, the dampness does under these circumstances take place, though the doors and windows are never opened. Shutting up a room when it thaws not sufficient.

In all cases, however, there is one method of preventing this species of dampness, which is infallible; and that is to keep every part of the internal surface of the wall in the chamber or staircase sufficiently warm by good fires. With this view all staircases ought to have some means of receiving artificial warmth. The wall should be kept sufficiently warm by fire.

If, notwithstanding this and the former precaution, a wall should accidentally become damp, the next best expedient is to dry it as quickly as possible by a free current of warm air. Dried if necessary.

This discussion, which at first sight might appear tedious and irrelevant, will, I trust, no longer be thought so, when it shall have been found necessary for the establishment of a principle on the subject more immediately before us.

In order to show the analogy, let us take the simplest example, which is that of a wainscotted room, unwarmed by fires. When the wainscot is colder than the air, it condenses the vapour in form of moisture. If that moisture were exposed to the influence of the sun and wind, the case would come under the former head of decay, which is that of Analogy in the case of dry-rot.

of wood wetted by rain in the open air. The water soon evaporates, and little decay proceeds in the wood. So in the wainscot, the surface next the room, though unprotected by paint, will perhaps be long in rotting, because the room admits of currents of air, more especially when doors and windows are frequently opened, so as to evaporate the superficial moisture, though less quickly and effectually than in the open air. But what is the case with the surface of the pannel next the wall? The air, loaded with moisture, penetrates into that interstitial space, and deposits it by condensation on that surface. But there is afterward no current of air to evaporate the water so deposited, which then slowly decomposes and destroys that surface of the pannel. Such is precisely the process of the dry rot, which always begins next the wall, and gradually proceeds to the painted or outer surface of the wood. It resembles in its chief circumstances the decay of paper in a damp room; and it precisely resembles that of paper projecting from the wall on canvas, which will still often happen, if the wall be subject to acquire a very considerable degree of coldness, though much more slowly than in the former case.

In wainscot.

Other cases similar.

The same process obtains in all other cases. Whenever the wood is cooler than the air which it touches, the vapour is condensed upon it; and being exposed to no new heat or current of air sufficient again to evaporate it, remains till another fit of condensation affords a new supply.

Thus the process of corrosion and decomposition is continually supported, till the wood moulders away.

Dry-rot an improper term.

The term dry-rot is, therefore, so far from being expressive of the real fact, that decay proceeds under these circumstances more quickly than in the open air, precisely because the wood is more constantly and uniformly wet; just as the lower parts of posts and rails, and any cavities in timber exposed to the weather, rot sooner than those parts which readily and speedily dry.

Cause of the smell.

The smell which we perceive on going into vaults or cellars, where this process is going on, arises partly from the extrication of certain gases, mingled perhaps with some volatile oil, and partly from the effluvia of those vegetable substances, which have already been said to grow on it; and which,

which, though they begin merely because the decayed wood is their proper soil, yet afterward tend probably to the more speedy decomposition of the wood itself. They cannot, however, with more propriety be said to be the cause of the dry-rot, than the white clover, which appears on certain lands after a top-dressing of coal-ashes, can be said to have produced the soil on which it flourished.

I have remarked above, that sometimes only a particular sort or sample of timber has in certain situations rotted, while another piece has continued for a great length of time perfectly sound. Hence persons have been deceived, and been disposed to attribute the dry-rot solely and universally to some original peculiarity in the wood itself. Dr. Darwin explains this fact by telling us, that the wood so decaying has probably been cut in the spring, when the sap in the alburnum was not only abundant, but of a saccharine quality; which, in combination with the vegeto-animal substance or gluten, disposes it to run with unusual readiness into destructive fermentation. In some trees, as by more particular custom the oak, the bark is a very valuable article of commerce, and is found not only to quit the tree more readily, but to contain a larger proportion of tan, in the spring, when the sap is rising, than at other seasons. Hence an old Act of Parliament, now in force, ordains that all oak, except for the purpose of building, shall be felled in the spring. Whether doors, posts and rails, paling, barrel staves, &c., come under the denomination of building, it may be difficult to say; but it seems at first view highly to be lamented, that any law should impose an obligation to destroy a valuable species of property. It would indeed be matter of peculiar regret, if an impolitic and avaricious spirit should induce the owners of oak forests to extend the same principle to the timber employed in the construction of great machines, and more especially the British navy.

Some have ascribed the dry rot to the nature of the wood.

Wood felled in the spring liable to it.

Various means have been employed in order to remove the tendency to the dry-rot in trees so felled. Thus they have been long exposed to the rain, or steeped, or even sometimes boiled in water, and then dried by artificial heat. These means do not however appear to have been successful

Means employed to remedy this.

in

in entirely washing out the fermentible sap, which therefore makes them much more subject to the decay of which we are treating. It may however still be doubted, whether it acts in any other way than by furnishing a disposition, which requires to be called into action by the same cause which operates in all other cases, moisture.

Instance of their inefficacy.

In proof of what I have stated, I have been informed by one of our Vice Presidents, that in a large vat or set of vats for beer, belonging to him, the staves formed of oak $2\frac{1}{2}$ inches thick, notwithstanding they were previously steeped in hot water, and then thoroughly dried, in a very short time underwent the dry rot, while others in the same situation continued unchanged five or ten times that period. It is highly worthy of remark, that the outside of these staves, which was painted, continued sound, and that the decay began on the inside, where, from the vats being at different times more or less filled, they were subject to the joint and successive influence of moisture and air.

Range of temperature in which it occurs.

I have mentioned above, that the putrefactive fermentation cannot take place except in certain temperatures, the lowest of which, according to Thomson, must be but little below 45 degrees of Fahrenheit's thermometer, and the highest within the degree which produces dryness by evaporation. The temperature most conducive to this effect has not, so far as I know, been ascertained, though much useful information on this head might be obtained from a set of well conducted experiments.

Theory of the dry-rot.

The following then appears to be the whole theory of the dry-rot; that it is a more or less rapid decomposition of the substance of wood, from moisture deposited on it by condensation, to the action of which it is more disposed in certain situations than in others; and that this moisture operates most quickly on wood which most abounds with the saccharine or fermentible principles of the sap. Let us see how this theory corresponds with the best known means of prevention, and what more effectual measures it may suggest.

Timber should be felled at a proper time, & well dried.

The first point is certainly to choose timber properly felled and well dried. And here, in order to prevent the injudicious fall of large oak timber, it may be of some consequence

sequence to know, that the bark of such timber contains much less tan than that of the younger and more succulent wood; and that this principle, together with the proper extractive matter, is considerably more abundant in the bark of the Leicester or Huntingdon willow, than in that of any oak. According to the experiments of Mr. Davy, 7½ lbs. of the former will go as far in tanning leather as 9 or 10 lbs. of the latter. It has however been asserted, that if an oak, or any other tree, which is stripped of its bark, be suffered to stand two or three years before it is felled, the wood will have acquired a very great degree of strength and durability. Bark.

Next, where it is practicable, a current of air should be frequently made to pass along the surface of the wood. This expedient seems to have been particularly attended to by the ingenious architects of our Gothic churches, who are said with that view to have left various openings in the walls between the two roofs of those edifices. In order also to promote evaporation, a certain degree of heat, such as that of air heated by the sun or fire, should, if possible, be from time to time applied. Cellars themselves ought to have some communication with the outward air by means of windows and shutters, or trap-doors. And that these may be for a short time opened in proper weather, so as to have a draught of air; and that no very low degree of temperature is necessary for the preservation of fermented liquors, provided that temperature be uniform, is evident from the practicability of keeping wine extremely well in cellars which are not damp, and in which, therefore, one or both of these circumstances must have taken place. The wood should be exposed to a current of air. Cellars.

The destruction of wainscotting may be long deferred by keeping in the apartment suitable fires.

Lastly, the dry-rot may in all cases be infallibly prevented where it is practicable to cover the surface of the wood, properly dried, with a varnish which is impenetrable and indestructible by water. With this view two or three coats of the composition before described should be laid on the dry wood, before it is erected or put together, and a third or fourth after it is put in its place; and proper means should be taken thoroughly to dry each successive coat of varnish. When the wood is dried, it should be covered with varnish.

In

In situations of this kind, what means of preservation are necessary must be employed at first; as it seems scarcely possible to renew them on fixed timber with any chance of benefit.

(To be concluded in our next.)

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Wernerian Natural History Society.

AT the last meeting of the Wernerian Natural History Society, Professor Jameson read an account of a method of constructing and colouring mineralogical maps. We cannot give a satisfactory account of this paper without drawings; we shall therefore only observe, that maps executed according to this plan show distinctly the figure of the cliffs, terraces, mountain ranges and mountain groupes: and the colouring affords a true and harmonious representation of the alternation, extent, and relative position of the different rocks that appear at the surface. Professor Jameson at the same time laid before the society a series of mineralogical queries, which he had drawn up with the view of directing the attention of mineralogists to the particular objects pointed out by them.

We have permission to communicate these queries to the public.

Mineralogical Queries.

ENGLAND.

Mineralogical queries.

1. Does the granite of Cornwall belong to the oldest or newest granite formation, or do both formations occur in that county?

2. Is the schorl rock of Cornwall disposed in an unconformable and overlying position in regard to the older primitive rocks; if this be its position, on what rock or rocks does it rest, and what are its other geognostic relations?

3. Does the serpentine of Cornwall belong to the first or second serpentine formation, and what are the imbedded and venigenous fossils it contains?

4. What are the characters of the different metallifarous venigenous formations in Cornwall: are any of them identical

tical with those described by Werner*, Mohs†, Friesleben‡, Jameson§, and others? Mineralogical queries.

5. Do the inclined slaty strata in the vicinity of Plymouth belong to the transition class of rocks?

6. Does the upper part of the mountain of Cader Idris in Wales belong to the newest flœtz trap formation?

7. Are not the mountains in Cumberland principally composed of transition rocks partially covered with the newest flœtz trap formation?

8. Is not the porphyry of Cumberland a variety of clinkstone porphyry?

9. Does the gypsum of Cumberland belong to the first or second flœtz gypsum formation?

SCOTLAND.

1. Does the sienitic greenstone of Fassnet burn in East Lothian belong to the transition rocks, or the newest flœtz trap formations?

2. Does clay stone occur in beds or veins in the coal fields of the Lothians?

3. What are the geognostic characters and relations of the porphyritic rock of the Ochil hills?

4. Is Inch Keith in the Firth of Forth entirely composed of rocks belonging to the independent coal formation?

5. Are the geognostic relations of the porphyry slate or clinkstone porphyry of East Lothian the same as in other countries?

6. What are the geognostic relations of the claystone, compact feldtspar, and striped jasper of the Pentland Hills?

7. What is the extent and mode of distribution of the sienite of Galloway?

8. Does the Craig of Ailsa in the Firth of Clyde and the Bass rock in the Firth of Forth belong to the newest flœtz trap formation?

* Neue Theorie von der Entstehung der Gänge von A.G.Werner, 1791.

† Beschreibung des Gruben-gebäudes Himmelsfurst. von F.Mohs, 1804.

‡ Mineralog Bemerkungen bei gelegenheit einer Reise durch den merkwurdigsten Theil des Harzgebirges, von Friesleben, 1795.

§ Mineralogical Description of Dumfrieshire, 1805. Elements of Geognosy, 1808.

9. Does

Mineralogical queries.

9. Does the pitchstone of Ardnamurchan belong to the newest flœtz trap formation?

10. Is the granular quartz in the islands of Isla and Jura subordinate to mica slate, or does it constitute a distinct formation?

11. Are the Cullin mountains in the isle of Skye composed of rocks belonging to the newest flœtz trap and second porphyry formations?

12. What are the geognostic characters and relations of the obscure egg in the isle of Egg one of the Hebrides?

13. Of what rock is the isle of Staffa composed, and what its geognostic characters and relations?

14. Is the porphyry of the isle of Rasay porphyry slate?

15. What are the geognostic relations of the tremolite of Glen-Elg in Invernesshire?

16. Does the upper part of Ben Nevis belong to the second porphyry formation; and if this be the case on what does the porphyry rest?

17. Does the porphyry of the Brauer near Blair in Athol belong to the first or second porphyry formation?

18. Does the granitic rock in the vicinity of Aberdeen belong to the granite or sienite formation?

19. Does the sandstone of the Shetland islands belong to the independent coal formation, or to any of the formations described by Werner?

20. In what species of mineral repository are the ores of Sandlodge in Shetland contained, and what are the oryctognostic and geognostic characters and relations of these ores?

21. Does the claystone of Papa Stour, one of the Shetlands, belong to the newest flœtz trap, or coal formations?

22. Does the serpentine of the islands of Unst and Fetlar belong to the first or second serpentine formations?

TO CORRESPONDENTS.

F. R. S. will perceive that the communication from Professor Vince, inserted in our Supplement, renders it less necessary to insert his favour. At the same time that his general remarks upon the spirit most desirable to be shewn in controversional writings must be allowed, it must be admitted in behalf of the Editor of a periodical publication, that very cogent and manifest reasons ought to present themselves, before he can be justified, for interfering in the discussions transmitted to the Public.

The Editor having been, contrary to expectation, disappointed of the Meteorological Register, is still obliged to postpone it: but he will take proper measures to prevent farther delay.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE, 1808.

ARTICLE I.

Observations on such Luminous Phenomena in the Atmosphere, as appear to depend on Electricity. By a Correspondent, **(R. B.)**

IT has been long since incontrovertibly established, that lightning is the electrical stroke between the clouds and the Earth, or between one cloud and another. All the differences of opinion therefore relate at present to its attributes or affections, which philosophers have not scrupled to investigate by the assistance of the electrical machine. But there are many circumstances, for an explanation of which we must have recourse to the great theatre of nature. Lightning is electricity.

The luminous appearances seen above the surface of the earth are, ignes fatui, lightning, shooting stars, fire balls, and the aurora borealis. Whether the first be an electrical phenomenon has not yet been satisfactorily ascertained, and indeed their cause may be said to be entirely unknown; but lightning and the aurora borealis are perfectly imitable by electricity; and it is highly probable, that an electric spark would exhibit the appearance of shooting stars and fire balls, if of sufficient length and remoteness to permit its figure and angular velocity to be perceived. It is also probable, Enumeration of luminous meteors.

The electric spark or fire ball.

that the electric explosion consists of a ball or cylinder of no great length, ignited by the compression of the air or gas, or other fluid it drives before it. Admitting this, the zigzag spark with ramifications may be considered as a fire ball continually throwing out detached pieces; the brush will be a fire ball broken to pieces, and the lightning will not differ from fire balls but in its vicinity to the Earth, and its velocity, which is perhaps greater. An artificial fire ball moving slowly has been seen once, and but once, by Warltire the lecturer. See Priestley's Electricity.

The velocity of disengaged electricity 23 miles in a second for lightning,

The magnificent experiments of Watson on Shooter's-hill, in which the shock was transmitted through great lengths of wire, teach us nothing of the velocity of disengaged electricity, as there is no proof that it has any known relation to that of the electric matter passing through conductors. Most persons think they can distinguish the direction of lightning, but this may perhaps be a deception. M. Marat* is the only philosopher that I know of, who has made any observation from which an inference of the velocity of lightning may be deduced; and he himself remarks, that it is attended with various causes of uncertainty. He measured the angular distance between two clouds, from one of which a horizontal flash of lightning flew to the other, and found it 30 degrees: the time was 20 thirds, and the distance determined from the interval of time between the flash and the report, was 10,000 toises. From these data he infers, that the velocity was 19,200 toises per second, which is somewhat more than 23 English miles.

and also for a fire ball;

This determination, by its remarkable coincidence with that of Sir Charles Blagden, respecting the velocity of fire balls, might lead to a conclusion, that there is a settled velocity for luminous electric matter, if it were not credibly ascertained, that it sometimes moves much slower, and is even nearly stationary, according to circumstances. In the storm which happened at Steeple Ashton†, on the 20th of June, 1772, two gentlemen being sitting in a parlour at the vicarage-house, and conversing about a loud clap of thun-

but it is not likely to be constant.

* Marat, Recherches physiques sur l'Electricité, p. 226.

† Ph. Trans. vol. 63, p. 292.

der

der that had just happened, they saw on a sudden a ball of fire between them, at about a foot distance from one of them. They described it to have been about the size of a sixpenny loaf, and surrounded with dark smoke; that it burst with an exceeding loud noise, like the firing of many cannon at once; and that they perceived a disagreeable smell, resembling that of sulphur, vitriol, and that of many other minerals in fusion. One of them was exceedingly hurt. As soon as he was struck he sunk in his chair, but was not stunned; his face was blackened, and his features distorted; his body was burned in several places, small holes were made in his clothes, and he lost in some measure the use of his legs for two or three days. He is positive he saw the ball of fire in the room for a second or two after he was struck. He also saw after the explosion a great quantity of fire of different colours, vibrating backwards and forwards in the room, with a most extraordinary swift motion. This might perhaps be an affection of his sight.

A fire ball in a room

Mr. Field, a painter of Trowbridge, during the storm, observed a ball of fire vibrate backwards and forwards over some part of Steeple Ashton, and at last dart down perpendicularly. This was in all probability the same ball as was seen to burst in the parlour of the vicarage-house.

seen before its descent.

A body of fire was also seen during the same storm moving towards a house, at some distance from the house of Mr. Paradise, which changed its direction and passed through the last house, and afterwards burst with a prodigious explosion. Mr. Paradise, who was three or four feet out of its line of motion, was struck against the wall, his body covered with fire, and he thought for some time he should have been suffocated with the smoke and smell of sulphur. He escaped unhurt, and his house received no damage.

Another in the same storm.

To these instances of electric matter which produced the effect of lightning, though its velocity was too small to prevent its figure being perceived, may be added, the very severe stroke of lightning, which killed two of the servants of Mr. Adair, at Eastbourne* in Sussex, threw himself hurt

Lightning at Eastbourne, of which the figure was observable.

* Ph. Trans. vol. 71, p. 42.

and motionless on the floor, and rendered a young lady and her servant insensible for a time, though these persons were in different apartments of the house, and left considerable marks of its violence on the house and furniture. It happened on the 17th of September, 1780. The morning was very stormy, with rain, thunder, and lightning; and just at nine o'clock a horrid black cloud appeared, out of which Mr. Adair saw several balls of fire drop into the sea successively, as he was approaching a one pair of stairs window; very soon after which, he was struck by a most violent flash of lightning, the effects of which may be particularly seen by consulting the original account. But what more especially applies to the present purpose is, that multitudes on the seashore before the house saw the meteor dart in a right line over their heads, and break against the front of the house in different directions; and all agreed, that the form and flame exactly resembled an immense sky rocket.

Distinction between lightning and the aurora borealis.

These facts show the near resemblance between lightning and fire balls. It is probable however, that the electric matter, when it passes violently through the lower regions of the atmosphere, usually has the form of a spark; that is to say, it passes with an extreme angular velocity in some definite direction. But the masses of luminous matter, which pass along the superior and more rarified parts of the air, appear either in the form of those flashes, which we produce by passing electricity through a vacuum, or in the form of balls of fire. In either case the phenomena are on a scale of astonishing magnitude.

Shooting stars, aurora borealis, and fire balls, are greatly elevated.

Shooting stars, the aurora borealis, and fire balls, have in general been found by the best observations to be greatly elevated in the atmosphere; and indeed, beyond the region where the action of the sun's rays on the air occasions the twilight. Mr. Brydone* frequently observed shooting stars from the mountain St. Bernard, one of the high Alps, and also saw several from the highest region of Mount Etna, and they always appeared as high as when seen from the lowest grounds. I find however one curious instance of lights resembling both the aurora borealis and shooting stars, at a much lower elevation.

* Ph. Trans. vol. 63, p. 167.

As

As Mr. Nicholson *, teacher of the mathematics at Wakefield in Yorkshire, was returning on horseback on the 1st of March, 1774, from Crofton, a village near Wakefield, he saw a storm approaching in the north-west quarter, from which the wind sat. It was then about half past six in the evening, and the weather was so dark and overcast, that it was with difficulty he could find his way. When the storm began, he was agreeably surprised to observe a flame of light dancing on each ear of his horse, and several others on the end of his stick, which had a brass ferule notched with using. These appearances continued till he took shelter in a turnpike-house.

Appearances in a storm resembling the aurora borealis

After having continued about twenty minutes the storm abated, and the clouds divided, leaving the northern region very clear; except, that about ten degrees high there was a thick cloud, which seemed to throw out large and exceedingly beautiful streams of light, resembling an aurora borealis, towards another cloud that was passing over it; and every now and then there appeared to fall to it such meteors as are called falling stars. These appearances continued till he came to Wakefield, but no thunder was heard.

& falling stars.

Do not these show two fluids like my sparks?

About nine o'clock a large ball of fire passed under the zenith, towards the south-east part of the horizon; and Mr. Nicholson was informed, that a light was observed on the weathercock of Wakefield spire, which is about 240 feet high, all the time the storm continued.

The present state of our knowledge respecting fire balls, with observations, is exhibited in an excellent treatise written by Dr. Blagden †, now Sir Charles, on occasion of the fiery meteors which were seen in the year 1783. The great meteor of Aug. 18, in that year, had the appearance of a luminous ball, which rose in the N. N. W. nearly round, became elliptical, and gradually assumed a tail as it ascended, and in a certain part of its course seemed to undergo a remarkable change, compared to bursting; after which it proceeded no longer as an entire mass, but was apparently divided into a great number or a cluster of balls, some larger than the others, and all carrying a tail, or leaving a train

Treatise of Sir Charles Blagden.

Great fire ball of 1783.

* Ph. Trans. vol. 64, p. 351.

† Ibid, vol. 74, p. 201.

behind.

behind. Under this form it continued its course with a nearly equable motion, dropping or casting off sparks, and yielding a prodigious light, which illuminated all objects to a surprising degree; till having passed the east, and verging considerably to the southward, it gradually descended, and at length was lost out of sight. The time of its appearance was 9h. 16m. P. M. mean time of the meridian of London, and it continued visible about half a minute.

Its height 57 miles; velocity 20 miles per second; diameter half a mile; course 1200 miles.

It seems probable, that the meteor burst and united again several times during its course; and that the great change corresponded with the period at which it suffered a deviation in its course. Its appearance was not uniformly bright, but consisted of livid and dull parts, which were perpetually changing their relative position. Its height deduced by computation from the angular elevations from various places, proves much more correspondent than might be expected from such data. One combination gives the height $54\frac{1}{2}$ statute miles, two give 57 miles, two 58, one 59, and one 60: the mean is $57\frac{1}{2}$ miles. It does not appear to have really approached the Earth in its course, which was above 1200 miles in length. Its absolute diameter across, supposing it to have been about half a degree broad, was half a mile, and its velocity was at least 20 miles in a second. A report was heard after its disappearance; and it is very remarkable, considering the rarity of the air at such a height, that the height of the meteor, deduced from the time of the passage of the sound *, nearly agrees with the geometrical deduction: it is $56\frac{1}{2}$ miles. A hissing, whizzing, or cracking, was also said to have been heard during its passage.

Sir Charles ascribes these appearances to electricity. The velocity greatly exceeds that of planetary projection

After describing the phenomena of the smaller meteor, which appeared on the 4th of October in the same year, Sir Charles proceeds to consider the cause of these phenomena. He shows the insufficiency of Halley's hypothesis, that they consist of a train of combustible vapours set on fire; and also of that which supposes they are terrestrial comets. This last position he observes is incompatible with their general appearance, which does not resemble solid bodies; with their exceeding great number, which could scarcely

* Ph. Trans. vol. 74, p. 111.

ly fail to produce some other appearances, beside a transient illumination; and more particularly with the extreme velocity of the meteor of Aug. 18, which is three times as great as a body falling from infinite space towards the Earth would have acquired, when it came within 50 miles of the Earth's surface. He therefore recurs to electricity, the only agent in nature with which we are acquainted, that seems capable of producing such phenomena. Its extreme and hitherto unmeasured velocity, the electric phenomena attending fire balls, the hissing noise, their connection with and similarity to the northern lights, which have sometimes assumed this form, and particularly their course, which is for the most part nearly in the magnetic meridian, are among the circumstances which are pointed out and elucidated in a perspicuous and highly interesting manner. And he concludes by observing, that if the conjectures he offers be just, there are distinct regions allotted for the electrical phenomena of our atmosphere. Here below we have thunder and lightning, from the unequal distribution of the electric fluid among the clouds; in the loftier regions, whither the clouds never reach, we have the various gradations of falling stars; till beyond the limits of our crepuscular atmosphere, the fluid is put into motion in sufficient masses to hold a determined course, and exhibit the different appearances of what we call fire balls; and probably at a still greater elevation above the earth, the electricity accumulates in a lighter less condensed form, to produce the wonderfully diversified streams and coruscations of the aurora borealis. Conclusion.

There is a fact observed by Mr. de Saussure, which seems difficult to be accounted for by the help of our present knowledge of electricity. He was on the Alps with some friends, while a thunder storm formed in the air beneath them. While it lightened and thundered below, they found themselves electrified, but differently, so that they drew sparks from each other*.

I shall finish this communication by a remark of Mr. Winn on the aurora borealis†, that this phenomenon is usually A south wind follows the aurora borealis.

* Memoirs of the Academy of Sciences for 1773.

† Ph. Trans. vol. 64, p. 128.

usually followed by hard southerly winds, with hazy weather or small rain; which Dr. Franklin, admitting the fact, supposes to be a consequence of the clearness to the northward, which renders them visible, and may have been produced by long continued winds from that quarter; for when the winds have continued long in one quarter, the return is often violent. The later discoveries respecting ignited stones which have fallen from the atmosphere, seem also to belong to the subject of this paper; but I cannot at this time consistently with brevity enter upon them.

II.

*Account of the Draining of the Pond of Citis**.

Draining ponds and marshes always considered a difficulty.

THE draining of ponds and marshes has always been considered as a difficult enterprise; and it has frequently happened, that works begun for the purpose have been relinquished, before the object was attained, either because the local circumstances have occasioned too many obstacles, the means employed have been inadequate, or the capital employed has fallen short, before the expected benefit could be derived from the undertaking.

A successful instance given as an example.

To instruct and encourage the speculator, as far as is in our power, and enable him to furnish agriculture with new land for the plough or the sithe, we hasten to publish the particulars of the draining of the pond of Citis, which is now going on. We shall point out the difficulties, that have been surmounted; and the new mechanical means, that have been employed.

Description of the pond.

The pond of Citis is to the south-west of the department of the Mouths of the Rhone, at a short distance from an arm of the sea called the Pond of Berre. It is near the ponds of Lavalduc, Pourra, Rassuen, &c. The different quality of the waters of these ponds, and the dissimilarity of their levels, show, that they have no subterranean communication with

* Journal des Mines, No. 116, p. 137.

each

each other, though they are so near. The pond of Citis is several feet lower than any of those here mentioned, and, what is very remarkable, it is near twenty-seven feet, English measure, below the level of the sea. This pond may be considered as a spacious basin, enclosed by lofty mountains, in which the rain water has accumulated and become stagnant, having no outlet.

Its level 27 feet below the sea.

The waters of Lavalduc are saline to sixteen degrees*. The proximity of this pond to that of Citis; the facility with which its water might be let into it, by opening a passage through the mountain separating them; and the decrease of the water of Citis after several years of drought, gave rise to the salt-works of Citis. These were undertaken by a company, who subscribed a joint stock to defray the expense. Their plan was to prevent the addition of more water, and gradually dry up the pond, by stopping on the sides of the mountains the course of the rain water, which was its sole supply. This attempt succeeded completely, and the affairs of the company were in a very prosperous way, when, after a memorable winter, the pond was completely inundated by the excessive rains, that fell for three months successively. The company indeed might blame themselves for this disaster; since by their negligence in not keeping the canal in repair, or rectifying its level, the rain-water, being so much more abundant than usual, could not flow with sufficient freedom through it; and thus by its weight breaking down the feeble dike that supported it along the sides of the mountains, it ran into the pond.

Salt-works established there.

These inundated.

This event, of which apprehensions had always been entertained, appeared to admit of no remedy to the company, who had long foreseen, that, if the pond should come to fill at any time, there would be no way to preserve the salt-works, but by carrying off the water over the hills between the pond and the sea. But what means could effect this? There appeared none but the common pump, or the screw of Archimedes; and these being too expensive or inadequate, the company was about to give up the work, when Mr. Augustus de Jessé proposed to drain it by employing a steam engine. Being admit-

Apparently a hopeless case.

Proposed to be drained by a steam engine, forcing the

* This I believe implies, that they contain 16 per cent of salt. Tr.

ted

water over a hill 172 feet high.

ted to present a statement of his design, he showed the practicability of conveying the water into the sea over the hills, though their tops were 172 feet above the bottom of the pond; and that, by adapting the power of the machine to the quantity of water to be raised, he could engage to accomplish it in a very short time. Lastly, as the company seemed undetermined, he agreed to undertake it at his own expense. His proposals and his conditions were accepted.

This might have been effected by a succession of steam engines;

Mr. Jessé might have accomplished his purpose, by placing several steam engines on the ascent of the first hill; the water raised by the first being raised higher by the second, and so on successively, till it reached the top. The power of these engines, which may be increased to any extent, assured him of a given quantity of water in a given time; but such a complication would have been detrimental to the general effect, for the draining could not have gone on regularly, unless all the engines had worked with constant uniformity, which could not easily have been effected. That he might have no obstacles of this kind, and no stoppage, he conceived the design, and carried it into execution, of throwing the water from the pond to the top of the first hill in a single stream, and by means of a single engine. This was adding to the difficulty; but in this the chief merit of the undertaking consists. We shall give an account of the works, by which this was accomplished: and we apprehend the reader will be gratified by the view of them given in Plates III and IV.

but a single one preferred.

The canal for carrying off the rain water first repaired and improved.

After having corrected the errours committed in the construction of the original canal, or drain for the rain-water, carried round the mountains, and encircling the pond, he raised its level considerably, so as to give it a greater descent toward the end where it discharged itself. This canal was supported in the steepest parts by stone causeways; and to prevent the fall of the water into it from being too forcible, he diverted it as much as possible from a perpendicular direction, giving it different inclinations, according to local circumstances.

A well sunk, with two pumps, worked alternately by

At some distance from the pond, on the slope of the hill, the steam engine is erected. A well is there sunk to a level below that of the bottom of the pond, and from its bottom a horizontal

horizontal gallery is carried to the pond at the distance of 320 feet. This gallery, or rather aqueduct, conveys the water from the pond into the well. For this purpose it was necessary, to arch it over completely. In the well are two pumps, and close to it is the steam engine, which works them both alternately by means of a double crank. Adjoining the pumps in the well are two vertical pipes, communicating with them, and united at the mouth of the well by means of an elbow, or fork. The part where they unite is fitted to a cast iron cylinder, 450 feet long, carried up the slope of the hill. This hill not being so high as some of the following, it was necessary to raise the cylinder upon supports of mason work to form a common level. A wooden trough, supported by tressels, unites the first hill to the second. This is 895 feet long. At the end of this trough begins a canal of 2494 feet, which is cut in the rock to the mean depth of 9½ feet. To unite the summits of all these hills it has been necessary to erect several aqueduct bridges, over which the canal is conveyed. The canal might have been cut to less depth, by raising higher the cast iron cylinder, and consequently the wooden trough; but the wind already has sufficient hold of both these, and they could not fail to have been weakened, had they been raised higher. If the iron cylinder had been made to rest on the hill, in order to dispense with the wooden trough, the canal must have been cut to an extraordinary depth, or a gallery of 2500 feet must have been cut through the rock, which would have occasioned an enormous expense.

a steam engine, and forcing the water through a cylinder 450 feet long to the top of the hill.

Thence conveyed by a wooden trough 895 feet long to the next hill; and by a canal with occasional aqueduct bridges to the sea.

The steam of the engine acts upon the pumps, which draw up the water of the well, and force it into the vertical pipes. These convey the water to the ascending cylinder, in which it rises gradually to the top of the first hill, whence it flows through the trough into the canal, which discharges it into the sea.

The water contained in the cylinder acts with all its weight on the valve, that separates it from the fork of the two pipes: yet such is the power of the engine, that at every stroke, of which it makes thirty-two in a minute, it not only raises a certain quantity of water into the vertical pipes, but gives it

The engine gives 32 strokes in a minute, raising 4660 lbs. of water in the cylinder.

a pres-

a pressure capable of raising the whole of the water in the cylinder, which is of the weight of 4660 lbs. avoirdupois.

Raises 69611 cubic feet, or 1942 tuns of water in a day.

The engine is calculated to raise 69611 cubic feet of water every twenty-four hours, making a weight of 38845 cwt. or 1942¼ tuns. It is obvious, that if it were required to raise a greater quantity of water, and at the same time to a greater height, as of 500 feet for instance, the same steps should be adopted, increasing proportionally the diameter of the cylinder of the steam engine, the dimensions of which give the measure of the power, and increasing the thickness of the cast iron pipe, so that it might be able to resist the pressure of the water forced into it.

Adequate means would produce a greater effect.

Novelties of the mode.

Before the draining of the pond of Citis, we do not believe a steam engine has been employed for such a purpose; still less pumps moved by the usual agents; or that any attempt has been made to raise a large quantity of water to a considerable height in a constant and uninterrupted stream. For this new application of it therefore we are indebted to Mr. de Jessé, and we trust that many enterprising persons will avail themselves of it. In the south of France, and near the coasts of the Mediterranean, there are a great many ponds, which it would be of importance to drain; their vicinity being a scourge to a country in other respects so much favoured by nature. Some attempts that have been made in the departments of the Aude and Gard enable us to presume, that the nature of the soil is in general excellent.

Places where it might be applied with advantage.

We conceive, that no draining can be attended with more difficulties than that of the pond of Citis; that Mr. de Jessé's method is applicable to any pond to be drained, attention being paid to local circumstances; and that is equally applicable to great morasses, the whole produce of which it would be so advantageous to obtain, at a time when the scarcity of fire-wood creates anxiety for the means of supplying the want of fuel.

Explanation of Pl. III, and Pl. IV, fig. 1.

Explanation of the plates.

A. The pond of Citis.

B. The arm of the sea, called the pond of Berre.

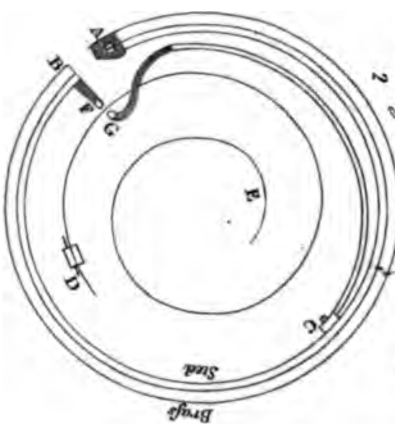
a. Level of the pond of Citis.

b. Level

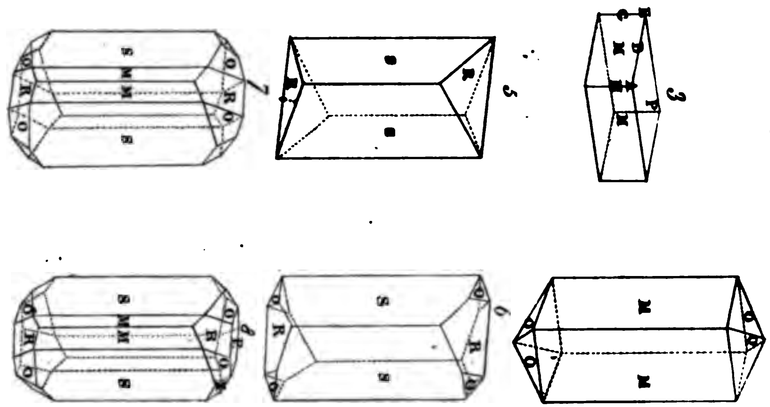
Draining of the Pond of Citis.

Begun the 15th of August 1805.

Mr Hardy's Compensation Curb.



Yenite



b. Level of the pond of Berre.

C D. Gallery that conveys the water from the pond of Citis into the well.

D E. The well, in which are the pumps.

E F. The cast iron cylinder.

F G. The wooden trough.

G H. The canal cut through the rock.

O. The steam engine.

g, *h*. Aqueduct bridges.

K K K K. Pillars of mason-work, supporting the iron cylinder.

K *m*. Height of the first hill.

III.

Remarks on some Pseudomorphoses observed in the Substances, that form Part of the Mineralogical Collection of the Council of Mines: by Mr. Tonnelier, *Keeper of the Mineralogical Cabinet to the Council*[*].

Minerals that crystallize regularly do not always appear under those figures, that may be considered as appropriate to them. Frequently they assume those of organized bodies, and sometimes those of substances included like themselves in the mineral kingdom, but of a different nature. These borrowed forms have been designated under the names of *pseudomorphoses*, or *pseudocrystals*; and these are the more suitable, because, if they do not always deceive us, they may at least under certain circumstances impose upon us with respect to their real origin. In some cases too they present us with enigmas not easy to explain, since we cannot always conceive what substance it is, the natural figure of which they have borrowed, though we soon detect those that have assumed it, under the mask by which they are concealed.

Stones assume forms not their own.

These called pseudomorphoses, or pseudocrystals.

The pseudomorphoses I have chiefly in view in writing these observations are thus far remarkable, that they appear in minerals,

Steatite and serpentine appear in the

[*] Journal des Mines, No. 116, p. 155.

phate of barytes*. The origin of these forms is by no means equivocal.

Pseudocrystals of quartz,

* In the collection of the Council of Mines are several quartzose pseudomorphoses, of which I shall content myself with mentioning the most remarkable. The first is borrowed from the metastatic carbonate of lime, and was found at Montbrizon, in the department of the Loire, by Mr. Laverrière, engineer in chief. The origin of this accidental form is by no means enigmatical. It is even necessary, in order to account for it, to have recourse to a sort of cementation, by which the particles of quartz would gradually have taken the places of those of the carbonate of lime, which before occupied the situation; it is sufficient, that a cavity left void by the calcareous spar, destroyed by any cause, served for a mould to the matter of the quartz. A piece of calamine, from Somersetshire, which is in the systematic collection of the Council of Mines, exhibit a pseudomorphosis similar to that of the quartz of Montbrizon. The pseudocrystals of this ore of zinc are of a reddish brown colour, three inches long, and hollow within, a circumstance in which they differ from the preceding, those being full and compact. The crystals of metastatic calcareous spar, which are sometimes found in the interior of those of calamine, and certain groupes of similar calcareous spar mentioned by Romé de l'Isle, part of which is still in the state of carbonate of lime, while the rest is in that of oxide of zinc, leave no doubt respecting the origin of this pseudomorphosis.

of calamine,

The department of the Saône and Loire, and that of the Nievre, visited by Mr. Champeaux, have afforded a variety of pseudomorphoses of a quartzose nature. These forms, all borrowed from acidiferous substances, derive their origin in some instances from fluate of lime, in others from sulphate of barytes. The regular forms borrowed from fluate of lime are the octaedron and the cube. These octaedrons are either hollowed out, or in relief. The faces of the first are plane, or convex: the second exhibit sometimes a regular octaedral summit, at others a simple equilateral triangle. The cubic forms, which are more numerous, are either solid or hollow. All these forms exist with the same appearances in the fluor spars found in the same place. The forms originating from the sulphate of barytes are the primitive form of that sulphate, with the trapezoid, the pointed, the laminar, the concrete, and the radiated varieties. The pseudomorphic quartz crystals originating from sulphate of barytes are not accompanied with this sulphate, as those indebted to fluate of lime for their form are with this fluate; whether because the sulphate of barytes has been subsequently destroyed, or because the pseudomorphic quartz has been removed from its place; which must have happened sometimes, since it is found not only in veins, but in ravines, and on the surface of the ground. However, on proceeding but a little way from the places where these pseudocrystals of quartz are found, we soon meet with veins of sulphate of barytes, and this in sufficient abundance, to leave no doubt of the origin of these pseudomorphoses.

of quartz.

If

equivocal. The fluate of lime, sulphate of barytes, and carbonate of lime, which are found in the same places, are so many faithful witnesses, which point out the source whence these forms are derived: and though we are not able to explain completely every circumstance respecting them, their nature cannot be doubted. When we find steatite exhibiting itself under several of the forms of carbonate of lime, may we not with great probability infer, that it has only imitated quartz by deriving from the same source the forms common to both? and when it presents itself under the forms that belong to quartz, is it not highly probable, that these forms are no more peculiar to it, than those of carbonate of lime are to quartz?

Steatite.

But it may be said, the crystals of steatite so perfectly resemble the mass in which they are enveloped, that we must suppose them to be the same substance, differing only in regularity of form. To this I would answer, such an inference is contradicted by analogy: for, when a substance is regularly crystallized, and its crystals are enveloped in an amorphous mass serving as their matrix, this is commonly of a different nature. Thus fine limpid crystals of hyaline quartz with two points are found buried in white Parian marble, in certain clays or marles, and in porphyries; crystals of hematoid quartz, or red jasper, and of borat of magnesia, are concealed in masses of gypsum; crystals of sulphate of lime are commonly found in banks of clay; crystals of specular iron ore, garnet, tourmalin, and magnesian limestone, occur in micaceous schist; &c.

Crystals do not form commonly in a mass of the same substance.

Instances.

It may be said farther, that the steatite, which exhibits forms analogous to those of rock crystal, presents others, that appear to be peculiar to itself; such for instance as the hexagonal prism with hexaedral pyramids truncated on the edges contiguous to the summit, which raises the number of terminal faces to twelve. This observation, I confess, might have been adduced as a very plausible objection, before quartz had shown

Steatite in a peculiar form:

but this has since been

If these pseudomorphoses of our departments be compared with these of Saxony, Bohemia, and Hungary, described by baron von Born, we shall find, that they present the same circumstances of form and situation, and have a similar origin.

found in quartz.

shown us in the crystals of the geodes of Oberstein this very secondary form, the structure of which, as ascertained by Mr. Haüy, is derived from the primitive rhomboid of quartz. But since this variety of form, which has not escaped the attentive eye of Mr. Tondi, occupies a place in the series of forms of quartz, the difficulty vanishes, analogy resumes all its weight, and the origin I ascribe to the regular forms of steatite retains it probability.

Argument from the laws of crystallization.

The laws of crystallization have been appealed to in favour of the opinion I combat. On breaking the steatite of Bayreuth, we discover in it parts, which have the form of the rhomboidal calcareous spar. It is in fact the primitive rhomboid of carbonate of lime, which has been mentioned above as one of the forms, under which steatite sometimes presents itself. Now it has been said, rhomboidal molecules are capable of producing the prismatic form of rock crystal, and that of the inverse calcareous spar, the muriatic calcareous spar of de l'Isle: therefore, the forms observed in steatite may be its own. It is very true, that the obtuse rhomboid of $101\frac{1}{2}°$, similar to that of carbonate of lime, performing the office of a nucleus and substractive molecule, may produce the hexaedral prism of rock crystal. It does this in the prismatic carbonate of lime, by means of a decrement on the inferior angle of the nucleus in which two rows of molecules are subtracted; and this law is general for every rhomboid. But it cannot produce the hexagonal pyramid, which terminates the prismatic hyaline quartz, with the same incidences which are constantly found in the quartz; as these require for the primitive form and subtractive molecule a slightly obtuse rhomboid only, the angle of which is about 94°.

Proof against this.

Carbonate of lime crystallized in the same figure, but with different angles.

Mr. Héricart Thury, engineer of mines, has found near Grenoble indeed carbonate of lime crystallized in a hexaedral prism with a pyramidal summit of six triangular faces; but this form has nothing in common with the prismatic quartz, the crystals being altogether different, both in respect to the incidences of the faces, and the values of their angles. It differs from the prismatic hyaline quartz, as the greenish yellow phosphate of lime in hexaedral prisms terminated by hexagonal pyramids, the *spargelstein* of Werner, differs from

the

the two former, and from the phosphate of lead, which sometimes assumes an analogous form.

In combating the opinion of those, who might be tempted to consider the regular figures under which the steatite of Bayreuth and the serpentine of Mont-Rose present themselves as crystalline forms properly belonging to these substances, I have not concealed the difficulties, to which the opposite opinion is obnoxious. I frankly confess the impossibility of conceiving, for want of local facts and observations, the means that nature can have employed for destroying the quartz crystals, which I suppose to have been originally included in the steatite, and fragments of which are found in neighbouring masses of steatite, to supply their place subsequently by a mass similar to the gangue in which they are included, yet so as to retain the ancient figure. I know not any rational explanation, to account for what has become of the substances, the forms of which alone remain. It appears to be a secret, which nature has preserved; but which farther observations, and inspection of the places, may perhaps some day enable us to penetrate. If however we believe the existence of nothing, except what we can completely explain, how narrow must be the bounds, to which we confine our knowledge!

Still it is difficult to conceive how the quartz crystals were destroyed, and the steatite assumed their place.

IV.

An Experiment on Soap-Suds as a Manure. By Mr. G. IRWIN, *of Taunton; with Remarks by the Rev.* THOMAS FALCONER*.

A Few years ago my attention was attracted by the soil of a garden, reduced to a state of poverty very unfriendly to vegetation. Interest in its future produce influenced my wishes for its restoration. An invigorating manure was necessary; but such a stimulus could not be easily procured. While considering which of the succedanea within my reach

Soil of a garden become poor

* From Papers of the Bath and West of England Society, vol. XI, p. 261.

had the greatest probable appearance of succeeding, it occurred, that possibly some trivial advantage might be derived from the oil and alkali suspended in the waters of a washing†. Pits were immediately ordered to be made, and in them the contents of a tub, which my servant usually committed to the common sewer, were carefully deposited: as washing succeeded washing, other pits were dug and filled; so that the whole garden, a small portion only excepted, has in this manner been watered and enriched: that small portion remains a visible demonstration of the utility of this manure. There vegetation is still languid; while the residue of the garden, invigorated by the suds only, annually exhibits a luxuriance almost equal to any thing this fertile neighbourhood can produce.

enriched by soap-suds.

I am, Sir, your humble servant,

GEORGE IRWIN.

Remarks, by the Rev. T. FALCONER.

Dr. Hunter's oil compost.

1. The above important experiment may perhaps remind the reader of the principal ingredients of the oil compost, suggested by Dr. Hunter of York. In the simple fluid manure we have an animal oil, potash, and water; in the compost are the same oil and the same alkali, but neither of them perhaps in so pure a state as in the manure, with the addition of "fresh horse-dung." The fresh horse-dung is added, in order to produce "heat and fermentation;" and a delay of "six months" is supposed to be necessary, to make the compost "fit for use." All, however, that seems to be gained by the horse-dung, is the animal oil, which may be united to the alkali during the process of fermentation, and the straw, which in the fermentation of the compost will bind the mass together, and when decomposed on the ground will afford a small supply of vegetable matter. If we make the comparison strictly accurate on the

† It is the common practice of some parts at least of the west of England, to use a lixivium, made by passing water through an appropriate strainer containing wood ashes, for the purpose of washing. This was probably the case here, though not mentioned by the author.

other

other side, we may observe, that in the fluid manure there must be an increased quantity of animal matter in the water, after it has been used for the purpose of washing linen.

The experiment then shows what is the advantage of the application of the oil and alkali only, as a manure, and perhaps the delay of "six months" in preparing the compost would not be compensated by any superior efficacy, that may be expected to arise from the combination of the horse-dung.

It also appears from the experiment, that the compost is a more useful discovery than Dr. Hunter himself could justly infer from his own limited experience of its effects.

2. This mixture of an oil and an alkali has been more generally known than adopted, as a remedy against the insects which infest wall-fruit trees. It will dislodge and destroy the insects, which have already formed their nests and bred among the leaves. When used in the early part of the year, it seems to prevent the insects from settling upon them; but whether by rendering the surface of the leaf disagreeable to the bodies of the animals, and thus repelling them, or by neutralizing the acid they deposit, and thus preventing the leaf from contracting into a necessary form for their reception, I cannot presume to determine. One of the modes, by which this mixture indirectly contributes to the fertility of the ground, may be by its destruction of the insects, which prey upon the plants.

Soap-suds a remedy against the insects that injure fruit-trees.

It is also, I think, to be preferred to the lime water, or the wood ashes and lime, which Mr. Forsyth recommends to be used for the removal of insects. It is preferable to the lime water and the lime, because lime loses its causticity, and with that its efficacy, by exposure to air, and must consequently be frequently applied; and to the dredging the leaves with the fine dust of wood ashes and lime, because the same effect is produced by the mixture without the same labour, and is obtained without expense.

Preferable to lime water, or caustic lixivium.

Mr. Speechley, in his treatise on the Vine, published in 1796, has used this mixture with great success; but he has applied it awkwardly and wastefully. He directs it to be poured from a ladder out of "a watering pot over both trees and

Mr. Speechley recommends it.

and wall, beginning at the top of the wall, and bringing it on in courses from top to bottom:" page 161. Mr. Speechley is not the first person who has thought of this application of the mixture. It is a fact which has been long known and neglected.

Best applied by a garden engine.

A considerable extent of wall may be washed by means of a common garden pump in a short time; and this operation should be repeated as often as a supply of the mixture can be procured; or if the water of a washing cannot be had, a quantity of potash of commerce dissolved in water may be substituted*. The washing of the trees and wall twice a week for three or four weeks in the spring will be sufficient to secure them from the injuries of these insects.

A valuable manure for the farm as well as the garden.

On the whole, then, this must be considered as a valuable manure, as it can be obtained easily, at small expense, and in large quantities; and, when its nature is well understood, will probably be no less esteemed by the farmer than horse dung. To the gardener, as well as to the farmer, it is useful, mixed with mould, as a fertilizing compost; or, when fluid may be applied to his fruit-walls, as a wash fatal to the noxious brood of predatory insects.

THOMAS FALCONER.

V.

An Inquiry into the Causes of the Decay of Wood, and the Means of preventing it. By C. H. PARRY, *M. D.*

(Concluded from p. 78.)

Would the varnish in some cases admit the growth of fungi?

I Do not know whether in very damp situations, surrounded with stagnant air, these varnishes would in time admit of the growth of fungi or mould. The brimstone might be sufficient to preclude that effect; but, if we believe Braconnot, seeds of the white mustard sown in pure flowers of brimstone, and well watered, became vigorous plants, which

* Mr. Speechley uses his mixture warm, to soak the shreds, and wash the wall, more effectually.

flowered

flowered and produced effective seed*. It is certain, however, that the essential oil of turpentine will act as a poison on growing vegetables; and perhaps the same property may exist in resin, which seems to be a similar essential oil, united with a certain proportion of oxigen.

The brimstone may defend from insects.

It is however highly probable, that the union of the brimstone may have another good effect, which is to prevent one of the causes of the destruction of timber which I have before mentioned, the depredations of insects. Whoever would learn the havoc, which certain animals of this kind are capable of making in hot countries, would do well to read Smeathman's description of the termes, or white ant, originally published in the Philosophical Transactions, and thence abridged into the English Encyclopedia Britannica, and other collections. In this country we know little of such ravages. Mischief however of this kind does sometimes occur, and may be the work of various animals, a particular account of which may be met with in the fifth volume of the Transactions of the Linnæan Society.

Coal tar.

I am informed, that in India, a circle of Lord Dundonald's coal tar drawn on the floor round boxes and other furniture, will effectually preserve them and their contents from the depredations of the white ant.

Other defences against insects.

It appears, that most insects are fond of sugar and mucilage; which is the probable reason why that wood is most subject to be penetrated by worms, which is felled when it most abounds with sap. In such cases, it might be well to try the effects of washing the wood, previously to the use of the varnish, with a solution of arsenic in hot water, in the proportion of 1 lb. to 10 gallons; or with a strong decoction of coloquintida or bitter apple, or white hellebore; after which the wood must be completely dried before the application of the varnish in the manner before directed. All these preparations are extremely cheap, and are either destructive or offensive to insects, and therefore will, probably, be an effectual defence against any injury from that cause.

C. H. PARRY.

Circus, Sept. 30, 1807.

* See Journal, vol. XVIII, p. 18.

VI.

VI.

Analysis of Jade; read to the Society of Natural History and Philosophy at Geneva, Dec. 5, 1805: by THEODORE DE SAUSSURE*.

General characters of jade.

UNDER the name of jade are generally comprised certain stones, not crystallized, remarkable for a greasy or oily appearance; a colour between waxy white and leek green, inclining sometimes to a blue, sometimes to a gray; a dull, greasy, scaly, and not lamellar fracture; extreme tenacity; hardness capable of scratching rock crystal; and lastly, a density superior to that of feldtspar or petrosilex.

Two stones possess these: the oriental, or lapis nephriticus;

Two stones, which have been considered only as varieties of the same species, unite all these characters in an eminent degree. One of these is the oriental jade, or *lapis nephriticus,* which Mr. Haüy calls *jade néphrétique.* This comes from China and the Levant, but we know not its situation in the earth. It is celebrated for the property ascribed to it by the Eastern nations of curing the renal colic, and allaying the pain of the stone. It is known in Europe only by the amulets, vases, and other pieces of sculpture brought from the places where it is native.

and one found in Europe, tenacious jade.

The other, considered by most mineralogists as a variety of the oriental jade, is found in several parts of Europe. My father was the first who made it known, after having found it on the borders of the Leman lake *(Voyages dans les Alpes,* § 112*)*, on those of the Durance, at Musinet near Turin, and in other places. From the name of the lake it was called *lemanite* by Mr. de la Métherie, who has well distinguished it from the oriental jade. Mr. Haüy has called it *tenacious jade;* and several authors have mentioned it by the name of Saussure's jade. This stone resembles the oriental jade in colour, hardness, tenacity, and fracture: but it differs in its specific gravity, which is greater; in its transparency, which is less; and in its fusion, which is more easy, and affords a perfect glass, with a smooth, conchoidal

Characters of this stone

Differs from the oriental.

* Journal des Mines, No. 111, p. 205.

fracture,

fracture, though frequently semitransparent, while the oriental jade produces only an opake mass, with a dull, uneven, and by no means conchoidal fracture. It differs likewise, as I shall show presently, in its constituent principles. It is proper therefore, that the name of jade should be taken from it; and I would propose to substitute that of Saussurite, as a compliment to the memory of my father, who first directed the attention of mineralogists to this stone. Names too, like this, which have no particular signification, are most convenient, because they do not lead us into error. Names derived from one of the places where a stone is found are always improper, as has frequently been remarked, because it is not peculiar to this place exclusively. Names derived from one of the characters of a fossil too, in whatever language they are framed, are not more suitable; since this character never belongs exclusively to the mineral denoted by it, which differs from others only by its general properties.

Name of Saussurite proposed for it.

Names should not have a determinate meaning.

Werner considers as a subspecies of jade the *beilstein*, *pierre de hache*, or axestone, which is chiefly known to us by means of the hatchets fabricated with it by the Americans. But this is much inferior in hardness and density to the stones generally comprised under the name of jade, and does not easily strike fire with steel; though it has a greasy appearance and greenish colour. On this stone however I can say nothing more, as I have it not in my possession, and have been able to examine it only superficially, so that I am obliged to leave its rank undetermined.

Beilstein considered as a jade.

The greasy polish of jades has appeared to most mineralogists to indicate, that they are impregnated with talcy particles, and that consequently they ought to be classed with the steatites. Mr. Hoepfner has confirmed this opinion by the analysis he has given of the jade of Swisserland. In this he found 0·47 silex, 0·38 magnesia, 0·04 alumine, 0·02 lime, and 0.09 oxide of iron. The magnesian nature of this stone appears the better founded, as it sometimes occurs in mountains of serpentine: but I thought it necessary, to repeat the examination, partly because this was made at a time when processes were less precise than at present; partly because

From their greasy appearance supposed magnesian: and Hoepfner's analysis gives 0·38 of magnesia in the Swiss jade;

but this questionable.

because the identity of the tenacious jade and the oriental jade did not appear to me to be proved.

Analysis of the oriental jade, jade néphrétique of Haüy.

Analysis of the oriental jade. The specimens described.

For this analysis I employed amulets cut in form of a crescent very little hollowed out. Their colour was a leek green, inclining to gray: their specific gravity 2·957. According to Brisson the specific gravity of this jade is 2·966*; and according to my father between 2·970 and 3·071.

These amulets are interiorly dull, and merely shining in small spots; they exhibit a dull fracture, with some fibres here and there, either straight or curved; they are semitransparent, and hard enough to scratch rock crystal, but are scratched by the topaz and the emerald. Their tenacity is very great: I could not pulverise them without greatly injuring an agate mortar, till I heated them red hot, and threw them into water. In a red heat they lose all their transparency and about $\frac{1}{100}$ of their weight, their green colour changes to a dark dirty gray, and they become fragile.

Exposed to a strong heat in a platina crucible.

1. One of these amulets, of the weight of about 6 grammes [93 grains], was exposed whole for an hour in a platina crucible to the most violent fire of a wind furnace. It there melted into a button, which was gray on the surface exposed to the air, but white interiorly; opake, being merely a little translucid at the edges; of a greasy, unequal, and confusedly lamellar fracture; and covered here and there with smooth, shining, greasy crystals, the extremity of which only was visible. This extremity exhibited very flat pyramids with four faces, the two larger of which terminated at the summit of the pyramid in two obtuse angles, and the two intermediate in acute angles. The upper surface of the button, when inspected with a microscope, showed a multitude of metallic globules of a gold colour, the nature of which I could not ascertain. The lower surface was covered with a row of large blebs, that did not penetrate into the substance. A small part of this button was fused before the blowpipe, but without forming a glass. One hundred parts of the jade by weight lost by fusion $2\frac{1}{4}$ parts.

* In Brisson's Mineralogy it is from 2·9502 to 2·9829. Tr.

2. I

2. I exposed to a red heat for two hours a mixture of 100 parts of this jade pulverised with 450 parts of potash. The result was a deep grass green mass, not vitrified, that communicated the same colour to cold water, in which it was diffused. This colour soon disappeared, the solution at the same time letting fall a gray flocculent precipitate, which afterward became brown. These defects indicated the presence of oxide of manganese, which for the present I left mixed with the other principles of the stone.

Heated with potash, and water affused.

3. The preceding liquor, as well as the undissolved part, was mixed with a portion of muriatic acid in excess; but this did not attack a brown or blackish flocculent residuum, which, being mixed with thrice its weight of potash, produced on exposure to the fire a green glass. This dissolved entirely in water and muriatic acid. The muriatic solutions being mixed and evaporated yielded a jelly, which being reduced to dryness, and the residuum digested in muriatic acid diluted with water, 53¾ parts of pure silex, distinctly characterized, were obtained.

Muriatic acid added.

4. The muriatic solution, separated from the silex, was mixed with ammonia; and a yellow precipitate formed, consisting of the metallic oxides and alumine. This precipitate, while still wet, was digested with potash twice in succession, to dissolve the alumine: but this solution, when supersaturated with acid and precipitated by ammonia, threw down but half a part of alumine.

The muriatic solution precipitated by ammonia.

5. The metallic oxides left on the filter after such a process as the preceding are seldom pure, as they retain both alumine and alkali. To separate these, they were mixed with five times their weight of potash, and heated red hot. The result was quickly diluted with cold water, and thrown on a filter, which retained the oxide of iron; a green liquor, holding in solution alumine and oxide of manganese, passing through. The oxide of manganese, precipitated by boiling the solution, weighed when dry half a part. The solution, after this oxide was separated from it, being supersaturated with acid, and precipitated by ammonia, some alumine was thrown down, which when dried at a red heat weighed one part.

Metallic oxides heated with potash, & cold water affused.

Manganese precipitated by boiling:

alumine by ammonia.

The oxide of iron, being freed from the alkali, that remained

Oxide of iron precipitated.

mained united with it, by dissolving it in muriatic acid, was precipitated by ammonia. After calcination it weighed six parts and half. But as its black colour indicated, that it still retained some oxide of manganese, I digested it repeatedly with vinegar, evaporating it to dryness every time, and redissolving the residuum in water. The solutions being added together, and precipitated by potash, yielded $1\frac{1}{2}$ part of oxide of manganese: the pure oxide of iron therefore weighed but 5 parts.

Some more oxide of manganese separated from it.

Carbonate of lime precipitated by carbonate of ammonia.

6. The muriatic solution (3) separated from the alumine and metallic oxides was supersaturated cold with carbonate of ammonia. This separated 22 parts of carbonate of lime, which furnished after calcination $12\frac{3}{4}$ parts of pure lime. The ammoniacal liquor, being filtered, let fall nothing on ebullition.

No magnesia could be discovered among it.

The $12\frac{3}{4}$ parts of lime I dissolved in sulphuric acid, and digested in water: they were found to have the same degree of solubility as sulphate of lime, and I could not discover, either by crystallization, taste, or any other sign, an atom of sulphate of magnesia.

Products.

Thus a hundred parts of nephritic jade yielded me on this occasion

Silex	53·75
Lime	12·75
Alumine	1·5
Oxide of iron	5
Oxide of manganese	2
Water	2·25
	77·25
Loss	22·75
	100.

From the great loss an acid suspected,

This loss being much too great to be ascribed to an error in the process, I repeated the analysis in the same manner, endeavouring in addition to detect the presence of any of the acids, that sometimes enter into the composition of minerals.

but none found.

After this examination, which was so far fruitless, though in other respects it confirmed the preceding, giving nearly the

the same results, though from different specimens, I sought to discover an alkali in the amulets, by employing nitrate of barytes to decompose them according to Klaproth's method. Examined for an alkali.

A hundred parts of nephritic jade were mixed with five times their weight of nitrate of barytes. This mixture I divided into four parts; and after having exposed the first to the action of the fire in a platina crucible till it ceased to swell up, I added to it the second, and so on with the rest. The whole, after having been exposed to a red heat for at least half an hour, exhibited a spongy mass of the colour of goose-dung. This was pulverized, and diluted with a large quantity of cold water. The mixture assumed a lilac red colour, which disappeared by a boiling heat, but returned on adding a few drops of muriatic acid, and again disappeared on adding a farther quantity of the acid, which gave the liquor a yellow colour. It contained a white insoluble powder, weighing 43 parts. This powder was exposed to the fire with four times its weight of barytes; and the spongy white substance thus produced dissolved completely in water and in muriatic acid, without exhibiting the colours mentioned above. Heated with nitrate of barytes. Water affused, and muriatic acid added,

The muriatic solutions having been mixed together, sulphuric acid was added in excess, which separated the barytes, and part of the silex. The solution precipitated by sulphuric acid.

The liquor was filtered, and evaporated, till all the muriatic acid was distilled off. The residuum moderately dry was digested in distilled water, which dissolved the whole, except the last portions of silex, and a little sulphate of lime. The muriatic acid driven off by heat. Residuum dissolved in water

The solution being filtered, ammonia was added, which precipitated the alumine and metallic oxides. and precipitated by ammonia.

These substances having been separated, the liquor remaining after filtration was evaporated, and the residuum heated to redness. This, which was of a whitish colour, weighed 56 parts. Being diluted with cold water, 16 parts of calcined sulphate of lime were separated by filtration. The alkaline sulphate therefore weighed after calcination 40 parts. The liquor evaporated, and earthy sulphates separated.

The

The alkaline sulphates separated by crystallization.

The aqueous solution of alkaline sulphate, being left to crystallize slowly, showed itself to consist of sulphate of soda and sulphate of potash. These salts when crystallized weighed 74 parts. The sulphate of soda after calcination weighed 24·6 parts; that of potash 15·4 parts. Assuming for these salts the proportions assigned by Kirwan, we find, that the stone contained 10·83 parts of soda, and 8·44 parts of potash.

Component parts of oriental or nephritic jade.

On putting together these results, we find, that 100 parts of nephritic jade contain

Silex	53·75
Lime	12·75
Alumine	1·5
Oxide of iron	5
Oxide of manganese	2
Soda	10·75
Potash	8·5
Water	2·25
	96·5
Loss	3·5
	100.

Differs from all other stones.

Hence the nephritic jade appears to have no resemblance to any stone hitherto analysed.

Analysis of the Saussurite, tenacious jade of Haüy.

Specimen of saussurite, or tenacious jade, described.

For this analysis I selected a rounded pebble, found on the borders of the lake of Geneva by my father, who considered it as a pure and well marked jade. Its colour was a deep leek green inclining to sea green. Its surface, polished on one side by art, on the other by natural attrition, was smooth, shining, oily to the sight, and greasy to the feel. Its fracture was dull, not lamellar, fine-grained, and with large scales. On the edges it was translucid. Its tenacity was very great, and similar to that of the nephritic jade. It easily scratched rock crystal, but was scratched by the topaz and the emerald. Its specific gravity was 3·261. That of specimens weighed by my father was 3·318, 3·327, and 3·389. It was free from diallage, or smaragdite, which is almost always

ways found disseminated in it. It had no perceptible effect on the magnetic needle.

A saussurite very distinctly marked yielded before the blowpipe a greasy, semitransparent glass, of a white or greenish colour: but the same stone, which in this way produced such a glass, being exposed to the most violent heat of a wind-furnace in a platina crucible for an hour, yielded a light brown glass, of the most perfect transparency, and free from blebs both within and at the upper surface. Some were seen in contact with the sides of the crucible. I thus fused about six grammes of saussurite, which did not lose by this operation any sensible portion of its weight*.

Fused into a pale semitransparent glass; & at a greater heat into a perfectly transparent brown glass.

I shall not detail the processes I employed to analyse this stone, since they were the same as those already described. I shall only mention, that, to separate the alkali, I attempted to treat the powdered saussurite with sulphuric acid, by boiling it on it, and evaporating to dryness. I repeated this process with the residuum six times, powdering it each time. But I could not by this process extract above 0·12 the weight of the stone, or deprive it of more than 0·02 of alkali. I then treated with nitrate of barytes, assisted by heat, the insoluble part, which had retained the metallic parts, because it had been calcined. The spongy matter procured by this operation was of a greenish gray. Cold water did not bring out the lilac colour, which had appeared on treating the oriental jade in the same manner. This colour was owing probably to the oxide of manganese, which exists in some quantity in the oriental jade, but was scarcely sufficient to be weighed in the specimen of saussurite, that I analysed.

Analysed as the other.

Attempt to separate the alkali by sulphuric acid unsuccessful.

* On this glass free from blebs I made one striking observation. It was, that the specific gravity of the stone previous to fusion is much greater than that of its glass. The specific gravity of the saussurite is 3·261: that of its glass is at most 2·8. The glass is softer than the stone, and easily scratched by it.

The glass lighter & softer than the stone itself.

A hundred

Component parts of the saussurite.

A hundred parts of saussurite afforded me

Silex	44
Alumine	30
Lime	4
Oxide of iron	12·5
Oxide of manganese	0·05
Soda	6
Potash	0·25
	96·8
Loss	3·2
	100.

It is neither a magnesian stone, nor a jade.

From these results it appears, that the saussurite is not a magnesian stone. It appears too, that it cannot be classed with the nephritic jade, as the alumine, which is in very small quantity in the jade, forms a considerable proportion of the saussurite; and the two stones likewise differ greatly in the alkali they contain.

The saussurite compared with feldtspar.

The saussurite contains a great deal more metallic oxide than feldtspar; their earthy principles however are the same: at least they succeed each other in the same order, the proportion of silex only being greater in the feldtspar, and the proportion of alumine less. Their external characters, if we consider the extremes of the two species, are totally different, but there are gradations between these, that bring them almost together. Thus that feldtspar, which my father called greasy (*Voyages dans les Alpes*, § 1304), and which is found crystallized in the green antique porphyry called *ophites*, and confusedly crystallized in nodules of variolite, does not always exhibit any signs of a lamellar structure. Its hardness is so great, that it readily scratches rock crystal; and like the saussurite it has a greenish and oily aspect.

If the granulous and scaly petrosilices be feldtspars, as analysis tends to show *, another link is added to connect them.

* See the analysis and description of the petrosilex of Pisse-Vache. *Voyages dans les Alpes*, § 1057.

I do

I do not intend by these gradations to confound the two stones: their elements, and their external characters, considered in the extremes, are sufficiently marked, to constitute distinct species. I would only remark, that they have shades of resemblance, which tend to confirm the results of analysis.

VII.

Remarkable Fact of an Increase of Temperature produced in Water by Agitation. In a Letter from JOSEPH READE, *M. D.*

To Mr. NICHOLSON.

SIR, *Cork, May* 8, 1808.

SINCE my communication on the increased capacity of water, I have been engaged with some experiments on *heat* excited by friction, one of which I beg leave to communicate through the medium of your Philosophical Journal, and hope it may not be esteemed uninteresting. I shall confine myself to a concise recital of the experiment, which if confirmed, is in direct contradiction to received opinion, that the agitation or friction of fluids cannot excite sensible heat.

Experiment.

The temperature of the apartment being 40°, half a pint of water, at a similar heat, was poured into a tin bottle-shaped vessel; into the aperture of which was inserted a thermometer, surrounded with chamois leather, and made to fit accurately, with its bulb nearly in the axis. After briskly agitating the vessel for a few minutes, to my extreme surprise I found the temperature of the water rose 8 degrees; and even after the apparatus was uncovered and laid at rest on the table, the water continued to rise for several minutes; proving the origin of the heat to be inherent in the fluid, and independent of any external causes. Anxious however to obviate every source of fallacy or objection, I prevented

Water at 40° rose to 48° by agitation in a closed tin vessel.

Repetition of the exp.

I prevented the communication of caloric by my hands, or of radiation from my body, by coating the tin vessel with many layers of woollen cloth carefully wrapped round it; over which there was a tin case, the entire nearly two inches in thickness, and covered externally with three wet towels. In the course of the experiment I dipped my hands frequently in snow water, and also sprinkled the towels.

Having repeated this experiment with similar results before the Rev. Mr. Hincks, Lecturer on Chemistry in the Cork Institution, I now venture to lay it before the public. Mr. Hincks on repeating the experiment in a glass bottle, found the heat of the vessel, by means of a thermometer placed between it and the covering, to be inferior to that of the enclosed fluid, and on a par with the atmosphere, which proves in a most satisfactory manner, that there could be no communication of caloric from the hands. Some extremely interesting conclusions may be drawn from this experiment. What is the cause of the increased heat? certainly not arising from a diminution of capacity. Is caloric material or immaterial? Is friction adequate to account for animal heat? Should this experiment on critical examination be found correct, these, and some other speculation on heat, will occupy a more extensive inquiry.

Sir, I have the honour to remain,

Your very obedient humble servant,

JOSEPH READE, M. D.

VIII.

Further Remarks on Professor VINCE's *Answer. By a Correspondent.*

Remarks on Prof. Vince's letter upon gravitation.

IT is not the "mathematical," but the literary "abilities" of this country, that will be impeached, according to Professor Vince's ideas, by the observations contained in his answer; since "the errours in the works of Dytiscus" consist, if his explanation is admitted, in having first mistaken a plural number for a singular; and secondly, in having wantonly

Remarks upon Prof. Vince's letter upon gravitation.

wantonly understood a term in its common and only correct acceptation. But it appears to me, that the passage, which is the first subject of his critical remarks, admits, beside the two alternatives which he discusses, a third sense, essentially different from them both: "the two first terms of the series" may possibly allude to the two first terms of the *only* two series which are to be found in the essay, these two terms having already been mentioned as *sufficient* for determining the force: and if the author will take the trouble of reperusing the whole of his essay, instead of trusting to his memory for its general tendency, he will probably be aware, that such *must* have been his original meaning. Two of the four terms thus obtained destroy each other immediately after their birth; the other pair conspire in the production of a joint issue (p. 18); and this their offspring is precisely that which is honoured with a place in the 18th section, as the representative not only of both its parents, but also of the whole of the unfortunate family; for we are expressly and very truly told (p. 19), that the terms omitted are so small, that they could make no sensible alteration in the result. Let them rest in peace. Let not the same hand which has bestowed on them a decent interment as dead in Philosophy, now drag forth their poor remains to stand in dumb parade under the banners of Logic.

His explanation not admissible.

The series which Professor Vince now introduces to our acquaintance, as willing to present us with its two first members, is *not even mentioned* in the essay, much less so stated as to make it possible to found any reasoning upon it. If "it was proposed" to take any "second terms" of such a series into consideration, the proposal was wisely confined to the author's breast: for why should they be considered, if they could "make no sensible alteration" in the result? The series $\frac{\alpha}{a^2} + \frac{\varepsilon}{a^4} + \ldots$ may certainly vary as $\frac{1}{a^2}$, if all the Greek letters after the first become inconsiderable, and our author has virtually confessed in his essay, that they do become inconsiderable.

Difficulty removed.

As to the difficulty of extending the law to the internal parts of the sun's substance, it is perfectly obvious, that the

law of the density, as well as that of the force, must be supposed to change at the surface of every material body, long before $\frac{Q}{a}$ can become equal to P.

Atoms to be considered as separate.

Professor Vince's "two independent circumstances" are both dependent on the supposition of the external action of the medium on a material aggregate of considerable magnitude, which it never could have been in the contemplation of Newton to advance: it would be idle to maintain the possibility of the hypothesis on any other ground, than that of the independent action of the medium on every atom of matter. Here therefore he is fighting with a shadow, and not with "the vaunting assertions and errours of Mr. D."

Improper use of the term density.

It is difficult to perceive the "necessity" of employing the term density, in order to convey the idea of the square of the cube root of the density, simply because this was the *power* of the density that was required for the author's purpose. The density of light or heat diverging from a centre in the form of projected corpuscles, may be very justly estimated by the number of particles falling on a given surface, for this simple reason, that their number is here a true measure of the density; while in the case of an elastic medium it is not a true measure.

Atmospheres.

The idea of the interference of different atmospheres must be considered as in some measure foreign to the question, since only one general ethereal medium of variable density is supposed to be concerned, and since the modifications of this medium, produced by the several celestial bodies, might easily coexist without any material interference or interruption.

Mistake of another author.

I must beg leave to observe, on the other hand, that another modern author appears to me to have been somewhat too hasty in asserting, that the law of gravitation may be derived from the supposition of an elastic medium, repelled by a *force* which varies inversely as the distance. If I am not mistaken, such a force would produce, according to the common laws of the operation of forces, a medium varying in density as some given power of the distance, and an apparent attraction increasing with the distance of the material bodies concerned.

I have

I have been informed, that the only intimation commonly given to the author of a paper which is not to be printed in the Philosophical Transactions is a simple letter of thanks, without any further notice respecting it. But the Society does not usually return thanks for a lecture read by appointment: hence therefore must have arisen the omission, which Professor Vince seems to think so inexcusable.

Custom of the Royal Society.

I am sorry that any of your correspondents should have considered my remarks as written in an improper spirit: you, I believe, were not of that opinion; and I can only say, that if that correspondent could have pointed out to me any objectionable expressions, I should most willingly have omitted them. My only motive was the wish to repel an unjust attack; my observations tended more to impute inattention than inability to the party concerned; and I am at this moment ready to allow that a very great mathematician may not only be materially mistaken, but may resolutely defend his errour, when it is discovered by another person; and that he may even have so short a memory, as to forget, while he is defending himself, what he had before written on the same subject.

Intentions of the author.

I am, Sir,

Your very obedient servant,

7 May, 1808. DYTISCUS.

IX.

Calculation of the Rate of Expansion of a supposed Lunar Atmosphere. By a Correspondent.

To Mr. NICHOLSON.

SIR,

IT has been a subject of inquiry among some who are attached to astronomical speculations, whether or no, if the moon had ever been possessed of an atmosphere equally dense with that of the Earth, she could have retained it, without a very sensible diminution, in consequence of the Earth's attraction, upon the supposition of the infinite dilatability of the

Inquiry into the effect of the Earth's attraction upon the atmosphere of the moon.

Inquiry into the effect of the Earth's attraction upon the atmosphere of the moon.

the air, with a density always proportional to the pressure. The inquiry involves a great variety of considerations, and it would be extremely difficult to make an exact calculation of all the particulars connected with it; but it may be shown from some general principles, that the diminution would have become perceptible to a spectator situated on the Earth, in the course of a few centuries.

Equilibrium of the joint atmosphere.

If a be the distance of the moon from the earth, and x the distance of any other point in the line joining them, the force of gravitation will be as $\frac{1}{x^2} - \frac{1}{70\,(a-x)^2}$; and the centrifugal force, arising from the revolution round the common centre of gravity, to be added for the terrestrial atmosphere, and to be subtracted for the lunar, being equal to the force of gravitation at the distance of the centres, the joint force f acting on the particles of the atmosphere will be as $\frac{1}{x^2} - \frac{1}{70\,(a-x)^2} + \frac{1}{70\,a^2}$, and $\frac{1}{x^2} - \frac{1}{70\,(a-x)^2} - \frac{1}{a^2}$ respectively: or, since f must be equal to unity at the Earth's surface, when x is equal to the Earth's semidiameter b, $f = \frac{b^2}{x^2}$ near the Earth, without sensible errour, and

$f = \frac{b^2}{x^2} - \frac{b^2}{70\,(a-x)^2} - \frac{b^2}{a^2}$, for the lunar atmosphere. Then the density being y, which may also be called unity at the Earth's surface, we have $-c\dot{y} = fy\dot{x}$, and it is obvious that c must express the height of a column of air of uniform density capable of producing the pressure by its weight, in order that $-c\dot{y}$ may be initially equal to $\dot{x}$. Hence we have $\text{H.L.}\frac{1}{y} = \frac{1}{c}.\int f\dot{x}$; but $f\dot{x} = b^2\left(\frac{\dot{x}}{x^2} - \frac{\dot{x}}{70\,(a-x)^2} - \frac{\dot{x}}{a^2}\right)$; therefore $\text{H.L.}\frac{1}{y} = \frac{b}{c}\left(d - \frac{b}{x} - \frac{b}{70\,(a-x)} - \frac{bx}{a^2}\right)$, d being, without sensible errour, $1 + \frac{b^2}{a^2}$. Now b is 3958, and c 5.28 miles, and at the moon's surface x is about $60\,b$, and $a-x\ \frac{3}{11}b$; whence $\text{H.L.}\frac{1}{y} = 685.69$. Again, when f vanishes,

nishes, and the density is least, $\frac{1}{x^2} = \frac{1}{70\,(a-x)^2} + \frac{1}{a^2}$, and x is nearly .825 a, whence H. L. $\frac{1}{y} = 724.31$; and this density is to the density at the moon's surface as 1 to the number of which the hyperbolic logarithm is 38.62, and the common logarithm 16.773: and supposing the density to be increased in any given ratio, the proportion will remain the same, the number c still indicating the height of a column equal in density to the atmosphere, thus condensed, at the Earth's surface.

General law of motion.

Now the expansion of the lunar atmosphere, supposing it to be equal in density to that of the Earth, and to extend to the point where the force f vanishes, which is the most favourable condition for its permanence, may be determined from this general principle; that the motion of the centre of gravity of any system of bodies, some of which are urged by a greater force in one direction than in another, must be the same as if the difference of the forces acted on the whole system, collected into the centre of gravity. Thus, if the pressure of the highly rarified air, at the termination of the supposed lunar atmosphere, which would have kept it in equilibrium, be removed, the elasticity of the column pressing on the moon will be by so much greater than its gravitation; and the centre of gravity of the column will be repelled, with a velocity as much smaller than that of a body falling at the Earth's surface, as the pressure removed is smaller than the weight of the column: but this ratio is compounded of that of the densities at the opposite ends of the column, and that of the force of gravitation, or rather the force f, near the moon's surface, to its force at the surface of the Earth, since the mass required to produce the given density, by its pressure, is as much greater, as the gravitation is smaller; and if we diminish in this proportion the space which a falling body would describe in a century, we shall have 514 feet, for the elevation of the centre of gravity of a column of the lunar atmosphere in that time.

Centre of gravity of an elastic column.

But in order to estimate the effect of such a change, we must calculate the actual height of the centre of gravity of a given column of an elastic fluid: and for this purpose we may

may suppose the attractive force uniform. The height of the centre of gravity is determined by dividing the fluent of $xy\dot{x}$ by the mass, or by $1-y$; but, since $-c\dot{y}=y\dot{x}$, $xy\dot{x}=-cx\dot{y}$, x being $=c$ (H.L.$\frac{1}{y}$), or, according to a mode of expression lately employed by one of your correspondents, $cm\,(\frac{1}{y}^{\frac{1}{m}}-1)$, when m is infinite; hence $-cx\dot{y}$ $=ccm\,(\dot{y}-y^{-\frac{1}{m}}\dot{y})$, of which the fluent is $e+ccm$ $(y-\frac{1}{1-\frac{1}{m}}y^{1-\frac{1}{m}})=e-cxy-cy^{1-\frac{1}{m}}$, or $e-cxy$ $-cy$; which must vanish when $y=1$ and $x=0$; consequently $e=c$, and the height of the centre of gravity is $c-\frac{cxy}{1-y}$; and when $y=0$, this height is equal to that of the column c, which for the Earth's atmosphere is 5·28 miles. and for the moon's as much greater as the force is smaller, that is, 27·75 miles. The centre of gravity being therefore elevated 514 feet, or $\frac{1}{285}$ of its height, in a century, the mean density of the column must also be reduced about $\frac{1}{285}$; but since a certain part of this elevation depends on the supposed acceleration of the more distant portions, which would produce no sensible effect in the neighbourhood of the moon, we cannot estimate the mean rarefaction of the part remaining more nearly in its original situation, at more than about $\frac{1}{300}$; and this will be reduced to about one fourth for the mean of the whole atmosphere, surrounding the moon on all sides: so that we may take $\frac{1}{1200}$ for the mean rarefaction of such a lunar atmosphere in the course of the first century.

Sensible effects.

So small a rarefaction as this would certainly not be directly observable at the distance of the Earth. Supposing that the atmosphere would be visible until its density became equal to a given quantity, the point, at which this density would be found, would be depressed only about 18 miles, if the whole density of the atmosphere were reduced to one half, and by a diminution of $\frac{1}{1200}$, only $\frac{1}{1200}$ of 27.75 miles, or about 120 feet. The effect of an atmosphere would however

ever

ever be more perceptible in the refraction, which would occasion an alteration in the apparent place of a star about to be eclipsed, and which would amount, in the case of the Earth's atmosphere, to 66 minutes. But the refractive density of the lunar atmosphere would vary nearly as the 134th root of the distance, instead of the 7th; and the deviation, instead of 66 minutes, would become 13′ 50″, one 1200th of which would be only $\frac{7}{10}$ of a second, which would still be imperceptible; although in two or three centuries, since the rarefaction would increase at first as the square of the time, it might perhaps be discoverable; and this would be considerably sooner than the decrease of the moon's apparent diameter could be observed. It is however scarcely probable, that so slow a rate of diminution could have reduced the lunar atmosphere from a density equal to that of the terrestrial atmosphere, to its present state, in the course of 10,000 years. Lunar refraction.

I am, Sir,

Your very obedient servant,

16 *May*, 1808. HEMEROBIUS.

X.

Experiments on Molybdæna: by CHRISTIAN FREDERIC BUCHOLZ. *Translated from the German**.

IT is near thirty years ago, that the immortal Scheele discovered in molybdena, as it was then called, a peculiar metallic substance, many of the properties of which he made known, as well as its action on several other substances. Metal in molybdena discovered by Scheele.

Several able chemists, as Pelletier, Heyer, Ilsemann, Richter, Hielm, Klaproth, Ruprecht, and others, have since turned their attention to the same subject: but the knowledge we have acquired from their labours is by no means proportional to the number of chemists, who have examined it, and the time that has elapsed since the disco- Since examined by several, but our knowledge of it imperfect.

* Journal des Mines, No. 106, p. 241. The original was published in Crell's Journal, Vol. IV, 1805.

very

Constitution of it in the native state doubted.

very of Scheele. If any one doubt of this, he has only to cast an eye over the different elementary works we have on chemistry, to be convinced of it. Who would not be surprised to see chemists still in doubt respecting the composition of molybdena as it is found native? Some consider it as a sulphuret, in which the molybdena is in the metallic state; while others assert, that they cannot find a particle of sulphur in it, and look upon it as a native molybdena. The smell alone however is sufficient, to convince us of the presence of sulphur in it. Let any one heat laminæ of the purest molybdena, the sulphurous smell, that will exhale, must prove to him, that it contains sulphur, if he have not lost the sense of smell.

Proportion of oxigen in the acid unknown.

Farther we are ignorant of the proportion in which oxigen is combined with the metal to form molybdic acid, though it has been so long known. The want of positive knowledge on these points has led me to think, that, if I were to undertake a series of experiments on molybdena, I should attempt a task of some utility, and that would contribute to augment and improve our knowledge of this substance. To my friend Mr. Haberlé I am indebted for the quantity of molybdena, that has enabled me to make these experiments.

First the existence and quantity of sulphur to be ascertained.

The first thing to be done was, I conceived, to remove all doubt respecting the presence of sulphur, and to determine its quantity. This I imagined would best be effected, by oxigenizing both the sulphur and the molybdena, and separating by means of barytes the sulphuric acid formed. But it was necessary previously to ascertain, whether the molybdic acid, which also forms a salt of no great solubility with barytes, would not occasion some errour in this computation.

I. *Experiments to determine the composition of the native sulphuret of molybdena.*

The native sulphuret contains no excess of sulphur, & no oxigen.

Exp. 1. Twenty-five grains of very pure chosen molybdena were reduced to a fine powder, and heated quickly in a small glass matrass. No sulphur was disengaged. The matrass when cooled contained a little sulphurous acid vapour, and the molybdena, which had been heated red hot, had scarcely

scarcely lost an eighth of a grain. This experiment shows, 1st, that the molybdena contained no excess of sulphur: 2dly, that the heat applied was not sufficient to expel the sulphur from it: 3dly, that there was no oxigen combined with it.

Exp. 2. The molybdena of the preceding experiment was put into half an ounce of pure nitric acid, the specific gravity of which was 1·22, and made to boil on a sand heat. The acid attacked the molybdena pretty briskly, but not so much as I should have supposed. To accelerate the operation, and prevent the sulphur from passing to the state of sulphurous acid, I added a drachm and half of pure muriatic acid, of the weight of 1·135, and a drachm of nitric acid. After boiling for an hour the whole was converted into a homogeneous mass of a milky whiteness, which was diluted with eight times its weight of water; the solution was filtered; and the sulphuric acid, that had been formed, was separated, by washing well both the residuum and the filter. Into the liquor, that had passed through the filter, a solution of muriate of barytes was poured. This occasioned a precipitate, which, being carefully collected, dried, and heated red hot, weighed seventy-two grains, and comported itself as pure sulphate of barytes. To determine the circumstances, in which this precipitate is possible, I made the two following experiments.

Treated with nitric acid,

muriatic acid added,

and sulphuric acid formed,

which was precipitated by barytes.

Exp. 3. Five grains of molybdic acid were mixed with two ounces of distilled water; twenty drops of muriatic acid, of the strength mentioned above, were added; the whole was boiled for half an hour, and the liquor was filtered. The solution had a very rough metallic taste; and solution of muriate of barytes did not render it turbid, though a little sulphuric acid produced this effect immediately.

Molybdic acid not precipitated by muriate of barytes.

Exp. 4. Five grains of molybdic acid and twenty grains of pure liquid ammonia were put into two ounces of water, and the mixture shaken, till the whole was perfectly dissolved. A solution of muriate of barytes being added, a copious flocculent precipitate immediately formed, which was afterward redissolved on adding a few drops of muriatic or nitric acid, and shaking the mixture.

Molybdate of ammonia formed,

and muriate of barytes added.

These experiments show: 1, that no indissoluble molybdate

Inferences.

date of barytes is formed, unless the molybdic salt be neutralized; and not when any free muriatic or nitric acid is present. 2, that the molybdena contains a large quantity of sulphur; for the 72 grains of sulphate of barytes, obtained from 25 grains of molybdena in the 2d experiment, represent nearly 24 grains of dry sulphuric acid; which indicate the presence of 10·2 grains of sulphur, or 40·8 per cent. After these preliminary operations I could proceed to a more accurate analysis.

Sulphuret of molybdena treated with aqua regia.

Exp. 5. A hundred grains of laminæ of molybdena, picked with the greatest care, were put into a retort with six drachms of pure muriatic acid, of the spec. grav. of 1·135, and $2\frac{1}{2}$ of nitric acid, equally pure, of the gravity of 1·22, and distilled on a sand bath with a gentle heat. After an hour's ebullition almost the whole of the fluid had passed over into the receiver. What remained in the retort was white, some gray flocks excepted. The liquor was poured back into the retort, half an ounce of fresh nitric acid was added, and when about a third was distilled over the gray flocks disappeared. The liquor that had passed over contained neither sulphuric nor sulphurous acid. The white mass was diluted with six ounces of water and filtered; and the residuum was repeatedly washed with the greatest care.

Precipitated with muriate of barytes.

In order to be well assured, that in precipitating the sulphate of barytes, which I was preparing to do, no molybdate of barytes should be thrown down with it, I added two drachms more of pure muriatic acid to the liquor, which held in solution but a small quantity of molybdic acid. This being done, I added a very pure solution of muriate of barytes, and sulphate of barytes was precipitated. The liquor being passed through a filter previously weighed, the residuum was put into eight ounces of water containing two drachms of muriatic acid, and well shaken; after which it was filtered again, and washed. After being exposed to a red heat it weighed 284 grains, to which 6 grains must be added, as the filter, after being thoroughly dried, had gained so much in weight.

100 grs. sulph. of barytes contain 32·5 of acid: 100 of sulph.

As 100 grains of sulphate of barytes contain 32·5 of sulphuric acid, this quantity must have contained 94·25 grains: and further, as according to my experiments 100 grains of sulphuric

sulphuric acid contain 42·5 of sulphur, it follows, that 100 grains of sulphuret of molybdena contain 40·56 of sulphur, which differs very little from the result of experiment 2. This agreement seemed to render it unnecessary for me to repeat the analysis: yet certain circumstances, which will appear farther on, induced me to commence a new one, that no doubt might remain. I took a hundred grains of powdered molybdena, put them into a mixture of three ounces of nitric acid and one of muriatic, and treated them as the preceding; with this difference only, that, by employing a larger quantity of acid in the first instance, it was not necessary to return into the retort the liquor that passed over into the receiver. The sulphate of barytes obtained in this instance weighed 288 grains, which, according to the preceding computations, contained 93·6 grains of sulphuric acid, and 39·78 of sulphur. Taking a mean between this result and the preceding, we may conclude, that, 100 parts of sulphuret of molybdena contain

acid, 42·5 of sulph. hence 100 of sulphuret of molybd. contain 40·56 of sulphur.

Experiment repeated.

This gave 39·78 of sulphur.

Sulphur	40
Metallic molybdena*	60

Medium.

Exp. 6. A hundred grains of sulphuret of molybdena, dissolved as the preceding, and distilled to dryness, were put into two ounces of pure liquid ammonia, diluted with an equal quantity of water. The mixture being shaken, in the course of a quarter of an hour the whole was dissolved, except a few yellowish flocks, which, collected on a filter, scarcely weighed a grain after being heated red hot. On boiling this precipitate in muriatic acid it was decomposed, and yielded 0·75 of a grain of silex, with ¼ of a grain of oxide of iron. The smallness of the quantity seems to show, that these two substances were accidentally present in the

Sulphuret of molybdena treated as before, and ammonia added,

about 1 gr. of silex and oxide of iron separated.

* Scheele supposed, that molybdena existed in the acid state in its ore: it was the doctrine of the French chemists, says Fourcroy, that corrected this mistake, by showing Guyton, Pelletier, and all the authors or partizans of the pneumatic theory, that Scheele produced the acid by burning the molybdena, and loading it with as much oxigen, as it could take up. Fourcroy's Chemistry, Sect. VI, art. IV, § 2.

In the subsequent part of this paper of Mr Bucholz it is shown in the most evident manner, that the molybdena is in the metallic state in its ore.

molybdena,

molybdena, and merely in mechanical union with the specimens analysed.

For the subsequent experiments I was desirous of procuring some quantity of molybdic acid. The processes I employed are mentioned in another work, here therefore I shall mention them briefly.

II. *Process for obtaining molybdic acid.*

To obtain the acid,

native sulphuret roasted in an inclined crucible.

I took 11½ ounces of native molybdena, the quartz adhering to which was pretty well separated, and put it into a large crucible, which was placed obliquely on the fire. A strong heat was first given, to kindle the sulphur, which was afterward diminished, and the matter was roasted, stirring it occasionally with a wooden spatula. A large quantity of sulphurous acid was evolved, and the mass was entirely covered with a crust of the purest molybdic acid, which was of a lemon colour on the fire, and of the purest silvery white when cold. By taking a little trouble, and using some precautions, the whole might thus have been converted into molybdic acid: but as this would have required a great deal of time and attention, I put an end to the process, when I perceived, that the greater part of the sulphur was volatilized, that a considerable quantity of molybdic acid was formed, and that, on leaving it exposed to a lower degree of heat, the mass began to agglutinate, and even to become fluid near the sides of the crucible. By this operation I obtained 8½ ounces of a gray shining mass, perfectly crystalline, which was of a whitish gray colour when powdered. Half an ounce remained adhering to the sides of the crucible, which could not easily be separated from it.

The acid may be separated by boiling it with an excess of soda, or digesting in ammonia, and precipitating by nitric acid, or expelling the ammonia by heat.

The pure molybdic acid may be separated from the mass by heating it with water, adding carbonate of soda till it ceases to occasion effervescence, and afterward boiling it with a little excess of soda: or the separation may be effected by digesting the mass in pure liquid ammonia, the heterogeneous parts, such as the quartz and oxide of iron, remaining undissolved. On pouring nitric acid into the neutral solution, the molybdic acid is thrown down. The molybdate of ammonia might be decomposed likewise by fire; in which case sometimes

times molybdic acid, sometimes molybdena in the metallic state, or at least approaching to it, is obtained, according to circumstances.

I employed the latter method, that with ammonia, as being the most advantageous. Previous trials had taught me, that three parts of pure liquid ammonia, of the specific gravity of 0·97, dissolve one of molybdic acid reduced to fine powder, and separate it from any impurity, that may be present. In consequence I powdered the produce of the preceding roasting; put it into a bottle with ammonia closely stopped; and left it to digest for twelve hours, shaking it from time to time. The acid disappeared, and two ounces of heterogeneous matter remained, containing still a little molybdena not decomposed. This residuum was boiled with two ounces of common nitric acid, and the molybdena was readily converted into acid, which was obtained perfectly pure by means of ammonia.

The latter method preferable.

3 parts of ammonia dissolve 1 of molybdic acid.

The ammoniacal liquor, in which the roasted mass had been dissolved, became a little turbid at the expiration of five hours, and assumed a yellow ochre colour. Five days after, the matter that occasioned this turbidness had subsided, and comported itself like oxide of iron. Part of the limpid solution was evaporated to dryness, and part of the residuum was heated red hot, to obtain from it the pure molybdic acid, as I had formerly done with a smaller quantity, but my expectation was frustrated. At the beginning the molybdate of ammonia turned blue; and it ended in assuming a metallic aspect throughout, even interiorly; the blue colour changed to a coppery red, and it had a similar appearance to products I had formerly obtained, which every thing indicated to be molybdena in the metallic state, or nearly metallic. The mass became oxided anew on the surface: but it was more agglutinated in those places, where the heat had acted most strongly.

A little oxide of iron precipitated from the molybdate of ammonia.

Effect of heat on the molybdate of ammonia.

III. *Experiments to ascertain the most advantageous method of reducing molybdena to the metallic state.*

Exps. 7 *and* 8. By the process just mentioned having obtained a mass, which every thing led me to suppose was in the

Attempts to reduce molybdena.

the metallic state, before other experiments had given me farther light respecting its nature, I resolved to employ the disoxigenizing action of ammonia, to obtain metallic molybdena. I took six ounces of liquid molybdate of ammonia, and evaporated to dryness. During the evaporation it diffused a smell much resembling that of vanilla. The saline residuum was pressed tight into a small glass of a convenient form, and covered with a layer of charcoal dust; the glass was bedded in sand in a crucible; and this was set on the fire. After the ammonia was volatilized, the glass was closed with a chalk stopple, the fire urged briskly, and the crucible kept for half an hour in a strong red heat, which melted the glass. After the whole had grown cold, a tolerably compact mass was found, easily reducible to powder, of a copper colour inclining in some places to blue, with a metallic lustre, and exhibiting crystalline laminæ. It weighed three drachms.

Ammonia expelled by heat: residuum covered with charcoal: and exposed to a strong heat. Product.

To see whether this mass would not fuse into a button in a stronger heat, I pounded it; rammed the powder, which was of a violet colour inclining to copper red, tight into a crucible lined with charcoal; covered it with a finger's breadth of charcoal powder; closed the crucible well; and kept it for half an hour at a white heat in a forge fire. After cooling the mass was agglutinated in places where it was most exposed to the action of the fire; but in the middle it was pulverulent, and had retained its colour.

Attempt to fuse it into a button, but merely agglutinated.

Exps. 9 *and* 10. Desirous of repeating the preceding experiments with a greater heat, I put some molybdate of ammonia into a Hessian crucible. After the ammonia had been expelled by a moderate heat, I covered the mass with a stratum of charcoal, closed the crucible, increased the fire to a white heat, and kept the crucible in this state half a hour. After cooling a compact mass was found of a violet brown colour, the lower part of which, that was in contact with the bottom of the crucible, and had consequently experienced a stronger heat, was tolerably consistent; it could not be reduced to powder without difficulty. This powder was of a violet colour, and appeared to consist of a multitude of small crystalline scales of a metallic brilliancy. The fissures that traversed the mass in every direction exhibited on their sides a great

Experiment repeated with a stronger heat. Product.

great quantity of larger scales, likewise of a violet colour, and of a very fine metallic lustre. The outside of the mass was in great part covered with scales also, but smaller, and exhibited a very pretty changeableness of colour. The upper part, which had been in contact with the charcoal dust, displayed some reflections of an indigo blue. The outside of the crucible was spotted with green in several places.

Several circumstances in the preceding experiment indicate, that the mass would have become more soft and compact, if it had been subjected to a more violent fire. In consequence I took a similar quantity of molybdate of ammonia, and exposed it to the action of the most violent fire for an hour. After cooling I found a mass weighing five drachms in every respect similar to the preceding; except that it appeared a little more compact, and the fissures were filled with scales, which were more crystalline aud in larger quantity. These scales, examined with a lens in a strong light, resembled polished false gold; as the largest did when viewed by the naked eye.

Fire apparently not strong enough.

Greater heat employed.

Result.

As the masses obtained in the four preceding experiments had a specific gravity varying from 4·5 to 5·67 according to their different densities, and this is the specific gravity assigned by many to metallic molybdena; as besides, on heating them red hot in contact with air, or with nitric acid, in which case oxigen gas is evolved, they afforded molybdic acid; and lastly as they had a metallic aspect; I was induced to consider them as reguli of molybdena, particularly as no one had ever yet spoken of such an oxide. But subsequent observations taught me, that these masses were molybdena in a peculiar state of oxidation, and that by the processes I had hitherto followed it would be impossible for me to obtain molybdena in the metallic state. It was necessary therefore, that I should attempt its reduction by other means.

Reasons for supposing the product metallic molybdena:

but it was a peculiar oxide.

Exps. 11, 12, *and* 13. I took four drachms of molybdate of ammonia, similar to the two masses obtained in the preceding experiments; powdered it; mixed it with olive oil, so as to make a thick pap; put it into a crucible; and heated it till the oil was burned. It was then pressed down; covered with a stratum of charcoal powder, and over this a little pow-

Attempts to reduce it by making into a soft paste with oil.

dered chalk; another crucible was put over it; the fire was urged to a strong white heat, and in this state it was kept for an hour and a quarter. After the combustion of the oil, the mass was pulverulent, of a deep blue colour approaching to black, and in some parts violet: and after it had undergone the strong heat to which it was exposed, it was entirely of an ash gray, and formed a mass of an earthy appearance, the parts of which had but little cohesion; the part in contact with the crucible scarcely showed the slightest indication of fusion; and thrown into nitric acid it produced a more considerable effervescence, than the products of the preceding experiments. The solution thus formed was at first reddish, and afterward became milkwhite. I added concentrated muriatic acid, and boiled to dryness, without any perceptible solution taking place. These circumstances led me to think, that the molybdena was entirely reduced, and that nothing was wanting, but to unite the particles into a button.

Product dissolved in nitric acid, and muriatic added.

Attempt to fuse it.

To endeavour to form this button, the mass obtained in the preceding experiment, which weighed $3\frac{1}{2}$ drachms, was pressed tight into a small crucible, and exposed anew for an hour and half to the most violent forge fire. This heat was so great, that the whole of the surface was vitrified, and the iron melted and burnt in three minutes. After cooling, the stratum of charcoal powder, with which it had been covered, was scarcely diminished. The molybdena had almost entirely preserved the same form as before; it was of an ash gray colour; its particles were but slightly agglutinated; and no mark of fusion appeared even in the part that adhered to the sides of the crucible. Its weight was $3\frac{1}{4}$ drachms as before.

Slightly agglutinated merely.

Heated again.

I took the same mass a third time, powdered it with six grains of charcoal, and exposed it again for an hour and half to a forge fire, which I endeavoured to urge as far as possible. After cooling, the mass had the same ash gray colour as before; on turning the crucible upside down it fell out without breaking; and it had a slight degree of consistency, yet notwithstanding it was friable between the fingers, and easy to pulverize. No mark of fusion could be discovered in the inside of the crucible; but the mass had lost six grains of weight, from part of it adhering to the sides. The mass being

Agglutinated more strongly.

Eagerly ab-

ing thrown into water, this fluid entered its interstices with avidity. sorbed water.

Exps. 14 *and* 15. To know whether the molybdic acid were capable of being reduced by the action of fire alone, without being mixed with any carbonaceous matter, I took a piece of the acid, which had been melted, and weighed 55 grains. This I placed in the midst of charcoal powder in a crucible, and exposed it for an hour and half to the same degree of heat as in the preceding experiment. The result was a tumid mass, which had not any more consistency than that of experiments 12 and 13. It was like that of an ash gray colour, and had lost eighteen grains of its weight, or in the proportion of 32·73 per cent. In nitric acid it comported itself in the same manner as the masses obtained in the preceding experiments. Two hundred and seventy grains of the matter obtained from the molybdate of ammonia roasted in experiments 9 and 10, being treated in the same manner, and kept a quarter of an hour at a white heat, gave a similar product to that of experiments 12 and 13. They had lost 78 grains, or 28·89 per cent.

Molybdic acid exposed to a strong heat imbedded in charcoal.

In a second trial 264 grains of the violet brown oxide, having been kept only half an hour at a moderately strong red heat, the result was a mass but imperfectly reduced. The interior was no way changed; the surface only was gray. Replaced in the fire, and kept at a white heat for half an hour, it was entirely reduced, and lost 74 grains, amounting to 28·03 per cent. Hence it follows, that the substance obtained from the molybdate of ammonia is very far from being in the metallic state.

Attempt to reduce the violet brown oxide.

These experiments show, that oxigen may be separated from molybdena by the action of fire alone when in contact with charcoal. They prove, that the reduction of the oxide and of the acid of molybdena may be effected without great difficulty. It remains to be seen, whether this reduction can be effected with larger quantities, and whether the molybdena cannot be obtained in a button.

Oxigen separable from molybdena in contact with charcoal by heat alone.

Exps. 16 *and* 17. I took ten drachms of molybdate of ammonia; put them into a glass which I placed in a crucible, and exposed to a red heat for half an hour; and obtained

Molybdate of ammonia heated; the brown

 a mass

oxide bedded in charcoal; a mass of violet brown oxide weighing one ounce. This mass I placed in a crucible, surrounded it with powdered charcoal, and exposed it for an hour to the most violent forge fire. *and reduced by a violent forge fire.* The result was a metallic mass, the different parts of which were more or less frothy, and more or less tenacious, but the tenacity was in no place such, but it might be beaten to powder. The exterior part was of an ash gray; in the interior, and even at the surface where cavities and depressions were formed, it had a truly metallic lustre, and was of a silver blue colour. The parts that had this lustre being pressed upon and beaten in a porcelain mortar were extended a little under the pestle, and this increased their brilliancy; but on continuing it they were reduced to a gray powder. Their hardness before trituration was greater than that of silver of nine pennyweights (0·75), since they scratched it.

Attempt to obtain it in globules. In order to obtain this matter in small melted parts, I pounded six drachms, which I pressed as strongly as possible into a crucible lined with charcoal, and exposed to the most violent forge fire for an hour and half. After cooling I found, that the mass was agglutinated, and reduced in bulk one fourth. I could not get it out without breaking the crucible. Those parts which had been in contact with the button and sides had considerable tenacity, but it was not the same with the surface. *Agglutinated, but not actually fused.* However, it could not be said to have been actually fused in any place, its parts being merely agglutinated by a commencement of fusion. Every where it exhibited a large quantity of scales, which were of a silvery white and a metallic lustre. Bruised on glass or porcelain, they acquired a medium lustre between that of tin and that of silver; but this disappeared in twelve or fifteen minutes. *A few small globules at the bottom, that had been fused.* At the bottom of the crucible appeared a few grains of molybdena, of the size of a pin's head, which had evidently been fluid. They were all over of a metallic lustre, and silvery white, like the scales already mentioned. The lower part of the mass of metallic molybdena, when beaten with a porcelain pestle, assumed the same lustre*.

Notwith-

* Ruprecht appears to have observed something similar. He says, that in endeavouring to reduce molybdena he obtained some small metallic grains

Notwithstanding the molybdena here possessed all the properties, that characterize metals, lustre, compactness, and even malleability, though in a slight degree, I could not obtain a well fused metallic button by exposing afresh to the most violent forge fire for two hours a piece of the mass already obtained, that weighed forty grains. A trial I afterward made with two ounces of brown oxide was attended with better success, than any I had yet obtained. I exposed this oxide to the most violent fire, well kept up, but for one hour only: and though the whole mass was not fused into a button, yet in some parts appeared pieces of one or two drachms, almost entirely fused, having a spherical surface, a white metallic lustre, and a much greater consistency than any mass I had yet obtained. On rubbing these metallic parts against a very smooth piece of porcelain, they assumed a lustre, which it would have been difficult to distinguish from that of silver. I must observe too, that this lustre remained for several days; while in my other trials it did not continue an hour, probably owing to the moisture of the air.

Most successful attempt at fusion.

From the experiments hitherto related we may infer:

General conclusions.

1st, That heat, in decomposing the molybdate of ammonia, causes the acid, in consequence of the disoxigenizing action of the ammonia, to pass to a slighter degree of oxidation, and gives rise to a peculiar oxide, some of the external characters of which have been noticed in the account of the 11th, 12th, and 13th experiments.

2dly, That the oxide and acid of molybdena are completely reducible by the simple action of fire, when they are placed in the midst of powdered charcoal, and that the metal then appears of an ash gray colour: but that, this metal being

grains, the least of which had the appearance of silver; and that the sides of the crucible had a coating of the same colour. He did not venture to assert however, that these grains were entirely metallic; for he observed others which were either of a whitish gray, reddish, or blueish. The following part of this essay, and what has already been said, will show, that these coloured grains belong to the oxide, which has been mentioned. Hielm, observing that molybdena rendered the colour of other metals lighter, inferred, that its own colour was white. This inference is confirmed by my experiments.

difficult

difficult to fuse, the most violent fire must be employed, to obtain a more compact metallic button. The experiments related put the possibility of this out of doubt.

IV. *Determination of the specific gravity of molybdena.*

Specific gravity of molybdena.

From the property, which the masses of molybdena I obtained in the metallic state possessed, of imbibing the water in which they are immersed, it is difficult to ascertain their specific gravity with accuracy. In the three trials I made, I suspended the masses by a hair to one of the arms of a very nice pair of scales; and in order to expel the air as much as possible, I boiled them for a quarter of an hour in distilled water. I afterward weighed them at the common temperature.

About 8·611.

The first trial gave a specific gravity of 8·636; the second of 8·490; and the third 8·615: we shall not be far from the truth therefore, if we take the mean term of 8·611. It is true the result differs much from that given by some authors, who fix the specific gravity at 4·5, or at 6·5. Hielm gives 7·5 for the maximum: but it is probable, that the masses, with which these were determined, were not pure, or were full of blebs, which occasioned the specific gravity to appear less than it really is to Hielm, Ruprecht, and Heidinger.

V. *Determination of the proportion of oxigen to metal in the molybdic acid.*

Experiment to ascertain proportion of oxigen in molybdic acid.

Exp. 18. The knowledge of the quantity of metal contained in the native sulphuret of molybdena affords a convenient mode of ascertaining this proportion. For this purpose I took a hundred grains of select scales of molybdena, put them into a small retort with acid as before, and distilled to dryness. Toward the end of the operation sulphuric acid was extricated in gray and heavy vapours. To expel this acid entirely, I broke the retort cautiously; put the pieces into a small glass, which was placed on a sand bath in a crucible; and kept them in a red heat for half an hour. The whole of the sulphuric acid was thus volatilized, and the molybdic acid was left pure in the form of small crystals of a yellowish white colour inclining to gray. This residuum weighed ninety

ninety grains. The pieces of glass having been weighed, they were carefully washed, and weighed again; when they were found to have lost a grain. Thus the hundred grains of sulphuret of molybdena had yielded ninety-one grains of molybdic acid: and, if we admit as above sixty parts of metal in the sulphuret, the proportion of metal to oxigen will be as sixty to thirty-one, or as 100 to 51·67; consequently 100 parts of acid contain 34·06 of oxigen.

Exp. 19. Desirous of making a counter trial, I took some of the substance obtained in decomposing molybdate of ammonia by heat, which at that time I considered as molybdena in the metallic state, and endeavoured to oxigenize it. Having poured on it nitric acid of the spec. grav. of 1·22, a brisk effervescence immediately took place, which continued for some time, without requiring the aid of heat: but as I was drying the oxigenized mass, it was suddenly thrown out of the vessel, occasioning a loss, that prevented me from going on with the trial. Another experiment.

I then repeated the operation, employing a very tall glass to contain the oxigenized mass, while I dried and melted it. A hundred grains, treated with ten drachms of nitric acid, produced a radiated mass, that weighed exactly a hundred and nine grains; and, if we add one grain for what may have remained adhering to the vessels employed, we shall have 110 grains of acid from 100 of the metallic substance. This repeated.

This result differed so widely from the preceding as to convince me, either that the substance I had taken for molybdena in the metallic state was not actually so, or that I had made some mistake in determining the quantity of sulphur contained, and the quantity of oxigen absorbed, by the native sulphuret of molybdena. I resolved therefore to repeat my experiments. The substance obtained by expelling the ammonia from the molybdate was an oxide.

I have already given the result of the second experiment I made to find the quantity of sulphur contained in the native sulphuret. What I did to verify the proportion of oxigen to metal in the formation of the acid consisted in taking a hundred grains of native sulphuret of molybdena, which I put into a mixture of one ounce of muriatic acid and three ounces of nitric acid; and, in order to prevent loss from any being Experiment with the sulphuret of molybdena.

being thrown out, I conducted the oxigenation in a tall vessel, which I placed first on sand, and afterward in a crucible, the sides and bottom of which were coated with chalk. By this process I obtained ninety grains of molybdic acid, which indicate fifty parts of oxigen to a hundred of metal; so that 100 parts of molybdic acid would consist of 66·67 metal and 33·33 oxigen.

The regulus of molybdena, which I had obtained in my preceding experiments, afforded me another mean of verifying the results.

Experiment by acidifying the metal itself.

Exp. 20. A hundred grains of the metallic molybdena of experiment 13 were reduced to very fine powder, put into a porcelain capsule, and thirteen drachms of pure nitric acid added. An extraordinary effervescence took place, and a great deal of nitrous gas was evolved. On evaporating, the matter, which was at first of a brownish yellow, passed gradually to a whitish yellow. In drying it became orange, and even blue in those places where the heat was the strongest. After it was well dried, and collected together, it was fused in a glass; and its weight was found to be increased thirty-four grains, which indicates 25·37 parts of oxigen in 100 of molybdic acid. It was thoroughly crystalline, and formed crystals of a silver white inclining to gray.

Variation in the proportion of oxigen indicated.

Was it owing to charcoal?

The change of colour just mentioned, which has not been remarked before, indicated a variation in the proportion of oxigen to metal. It appeared to me probable, that a portion, though small, of the charcoal, which had been mingled with the molybdena to promote its reduction, had combined with it, produced the phenomena observed during the oxigenation, and changed the proportion of the oxigen to the metal.

Two attempts to ascertain this frustrated.

Exp. 21. To verify this suspicion, I thought it necessary to repeat the trial, employing molybdena that I had reduced by simply placing the mass to be reduced in the midst of powdered charcoal, without having triturated and mixed them together. The experiment failed twice. The first time the effervescence and swelling up on pouring the nitric acid on it were so great, that the matter ran over the sides of the vessel: and the second, though the acid had been diluted,

luted, explosions took place, by which some of the molybdic acid was thrown about.

To prevent these accidents, I made the trial again in wide-mouthed capsules. I took a hundred grains of powdered metal, and poured on it an ounce of nitric acid diluted with half an ounce of water. In a few minutes a very powerful action took place, and the liquor became of a yellowish red inclining to brown. The whole of the metal not being dissolved, when no more gas was evolved I added half an ounce of acid, and placed the solution on a sand bath. The whole was now dissolved, but the liquor remained of a yellowish red inclining to brown as before, only a reddish white powder appeared swimming in it. I evaporated the mixture to dryness, stirring it constantly. The residuum had a red copper colour, mixed with a great deal of white; on continuing the heat the surface acquired a grayish blue, the edges a brown red, and in some parts an orange yellow. A third

Thus it was evident, that the difference of colour exhibited by the oxigenized molybdena was not occasioned by charcoal mixed with it, but might be ascribed to different degrees of oxidation of the metal. It is surprising, that the molybdena should become oxigenized so imperfectly in this manner; while its oxigenation is accomplished much more speedily and completely, when sulphuret of molybdena is employed. To produce a perfect oxigenization, I poured half an ounce of nitric acid on the dry mass, and heated it. Finding no perceptible change take place, I added two drachms of pure muriatic acid, which expedited the effect, the mass became more and more compact and lighter coloured, and at length was white. Being dried and carefully collected, it was kept at a red heat in a glass capsule for a quarter of an hour, left to cool, and weighed. Its weight was now 145 grains, to which must be added three for what adhered to the sides of the capsule, so that in this experiment a hundred grains of molybdena had absorbed 48 grains of oxigen. In 100 parts of acid therefore there are 32·43 of oxigen. showed that charcoal was not concerned in it. The sulphuret more easily acidified than the metal.

This experiment then gives a result differing little from those obtained in the 14th and 18th. I repeated the first once more. Taking a piece of fused molybdic acid, that Exp. 14 repeated.

weighed

weighed 100 grains, I put it into a crucible in the midst of powdered charcoal, and exposed it for an hour to the most violent forge fire. The gray mass I obtained weighed exactly 32 grains less than the acid employed.

Molybdic acid contains about 67·5 of metal, and 32·5 of oxigen.

Thus we may admit, without being far from the truth, that a hundred parts of metallic molybdena absorb forty-nine or fifty parts of oxigen, when this metal passes to the acid state, and that consequently a hundred parts of molybdic acid contain thirty-two or thirty-three of oxigen.

This confirms the proportions of the sulphuret.

These experiments on molybdena in the metallic state, and on molybdic acid, confirm the proportions I have assigned to the constituent parts of the native sulphuret of molybdena.

(To be continued.)

XI.

Construction of a Curb affording a Compensation for the Effects of Heat and Cold in Time-pieces. By Mr. WILLIAM HARDY, *No.* 29, *Cold-bath Square.*

Compensation curb described.

IN Pl. I[illegible], fig. 2, the circles A B denote an expansion bar, composed externally of brass and internally of steel; in consequence of which its curvature will be diminished by heat, and increased by cold.

Manner in which it acts.

The end A is fixed, and the rest of the bar is at liberty, whence the extremity B will be affected by a motion from temperature, which will carry the pin F (between which and G the spring D E passes) a little onwards when the temperature rises, and a little backwards when it falls. But F will be scarcely at all affected in the direction of the radius, because the two semicircular parts of the bar counteract each other. C G is an arm of brass fixed to the expansion bar, in the middle of its length. It carries a pin G opposite to F; and as C is thrown inwards by increase of temperature through a space, amounting to the whole change to which the diameter of the curved expansion bar is liable, G will be carried towards F, and will allow the spring less play; and by that means add to its

The balance

stiffness,

stiffness, when its elastic force is diminished by the weather: and the contrary effect will take place when the spring becomes more rigid by cold. spring will have less play in hot weather.

An adjustment for temperature has been applied in chronometers several years ago, by means of a pair of tongs, or double expansion bar, of the same nature as if the spring were to pass through the variable notch A B in the present figure. Whether Mr. Hardy's contrivance will afford superior advantages in its effect, or in the convenience of disposing the parts, must be referred, like all things of this nature, to trial. A former adjustment on a similar principle.

W. N.

XII.

Of the Yenite, a new Mineral Substance: by Mr. LE LIEVRE, *Member of the Institute, Counsellor of Mines, &c.**

WHEN I was sent to the isle of Elba five years ago as commissary of the government, I thought I might avail myself of the opportunity, to study and make known the mineralogy of a country so interesting to the naturalist. The business of my office, however, having occupied nearly the whole time I spent in that island, did not allow me to carry this design into execution: but my journey will not have proved altogether fruitless to the science of mineralogy; for, beside the mineral that forms the subject of the present paper, I have brought with me some others, that may prove interesting to the mineralogist. Such are Minerals from the isle of Elba.

1. A green substance, that has some resemblance to actinote, and a considerable analogy to that which I am about to describe. A new mineral.

2. Transparent white emeralds, some of which are three centimetres [1·18 inch] long. White emeralds.

3. Black, yellow, and rose-coloured tourmalins. Tourmalins.

4. Rose-coloured and white lepidolite, lamellar and compact. Lepidolite.

* Journal des Mines, No 21, p. 65. Extracted from a paper read at the meeting of the Institute, December the 29th, 1806.

5. A

Porphyry. 5. A porphyry with base of white compact feldspar, and containing black globular nodules, which appear to me to be a mixture of amphibole and feldspar.

Diallage. 6. Green and metalloïd diallage.

Resinite. 7. Resinite quartz similar to that of Musinet in Piedmont.

Fetid quartz. 8. Fetid pseudomorphous quartz, &c.

In some future papers I shall mention what I have noticed, that is peculiar, either in the characters or situations of these: but I shall confine myself for the present to that, which I now lay before the class, and to which I have given the name

Yenite. of *yénite,* from of one of the most memorable events of the age, the battle of Jena.

Its physical characters.

Spec. grav. This mineral weighs nearly four times as much as distilled water (3·825, 3·974, 3·985, 4·061).

Hardness. Its hardness is a little inferior to that of the adularia feldspar, by which it is scratched; but it scratches glass strongly, and gives a few sparks with steel.

Primitive form. The mechanical division leads, as will be more particularly described presently, to a rhomboidal prism of 113° and 67°, which may be subdivided parallel to the shorter diagonals of its bases.

Colour. The yenite is opake, and of a black colour inclining sometimes to brown. Its powder is of the same hue.

Surface. The surface of the crystals, when they are very black, is shining. (Those varieties represented Pl. IV, figs. 5 and 6, have commonly a dull and brownish surface.) The lateral faces of the prisms are streited lengthwise: the facets O of the summit are smooth and very shining.

Fracture. The fracture is unequal, and of a greasy lustre (nearly like that of phosphate of manganese).

Nonelectric. It is not electric, either by heat or friction.

Magnetic when heated. Heated to redness in the flame of a wax candle merely, it becomes weakly attractable by the magnet.

Spontaneously decomposed. Exposed to the action of the air it is decomposed, and covered with an earthy yellow and brown crust, perfectly similar to the ochres, or oxides of iron mingled with earths, which are found native.

Geometrical

Geometrical characters *.

Cleaving exhibits indications of laminæ parallel to the sides of a prism with a rhombic base, the angles of which are 112° 37′ 9″ and 67° 22′ 51″. Rather more evident indications are found of a division according to the shorter diagonals of the rhombs. This section is pointed out on the crystals by the striæ at the summit. The bases present no section: their fracture on the contrary is uneven conchoidal, &c. Mechanical division.

The primitive form, Pl. IV, fig. 3, is a right prism, with a rhomboidal base, the diagonals of which are to each other as 2 to 3. From the theory of decrements its height is to the shorter diagonal as 4 to $\sqrt{7}$. Primitive form.

The crystals have five varieties with respect to figure. Figures of the crystals.

Var. 1. Fig. 4 is the primitive form elongated, and terminated by a pyramid with four faces rising from its edges. The angle of incidence between M and O is 128° 28′ 59″; that between O and O, 139° 36′ 48″; and that between O and its reverse 117° 38′ 8″.

Var. 2. Fig. 5 is a tetraedral prism, nearly rectangular, terminated by a double bevil, obtuse, and placed on the obtuse angles. The angle of incidence between S and S is 83° 16′ 4″; that between R and its opposite face 113° 2′ 9″.

Var. 3. Fig. 6 is the preceding form with a double truncature at each acute angle of the bevil. The angle of incidence between O and R is 159° 48′ 24″.

Var. 4. Fig. 7 is an octaedral prism terminated by an obtuse octaedral summit, four of the faces of which are at the angles of the prism, and four on the edges. The angle of incidence between X and the edge Z is 131° 24′ 37″.

Var. 5. Fig. 8 exhibits the preceding variety with this difference, that it has at the summit a facet parallel to the base of the primitive form. The angle of incidence between P and R is 146° 31′ 43″; and that between P and O, 141° 31′ 1″.

* These characters were ascertained by Mr. Cordier, engineer of mines, who was so obliging as to undertake the examination of the crystalline forms, which he calculated according to the method of our learned comrade Haüy.

At

Differs from all others.

At first view this mineral seems to approach the epidote in form: but in the first place the regularity of the faces forms an objection; and in the second, the measure of the angles, and the laws of decrement, totally contradict this apparent analogy. No other mineral substance has any similarity with this new species, at least as far as respects form.

Chemical characters.

Chemical characters.

The yenite simply calcined becomes attractable by the magnet, passes from black to a very dark reddish brown, and loses about two per cent of its weight.

It readily fuses before the blowpipe, without any sensible ebullition; and yields an opake, black button, very readily attracted by the magnet, but without polarity, dull, and exhibiting a metallic aspect. With glass of borax it dissolves with a short effervescence. On continuing the fire an enamel is obtained, that appears black: if a larger quantity of borax be added, we have a transparent glass, of a yellowish green colour, without any indication of a metallic button, or residuum; which proves, that the whole has been dissolved.

It is attacked by the sulphuric, nitric, and muriatic acids. The last dissolves it most readily: the silex remains at the bottom, and the solution acquires a fine yellow colour, with a slight tinge of green.

Analysis.

It has been analysed by Messrs. Vauquelin and Descotils, and a hundred parts gave

Component parts.

	Descotils.	*Vauquelin.*	
Silex	28	29	30
Lime	12	12	12·5
Oxide of iron	55	57	57·5
Oxide of manganese	3		
Alumine	0·6		
Loss	1·4	2	
	100.	100.	100.

This

This agreement between the results obtained by these two skilful chemists, who operated on the stone at the same time, and unknown to each other, gives these analyses as high a degree of certainty as could be wished; and authorises us to conclude, that the yenite, at least in those specimens analysed, contains rather more than half its weight of iron, mixed with a little manganese, and that the rest of the stone is lime and silex, the proportion of silex being considerably more than double that of the lime.

Situation and local circumstances.

I found the yenite in two different places in the island of Elba; at Rio-la-Marine, and at Cape Calamite. Where found.

In the first of these it forms part of a very thick mass or stratum, resting on a primitive limestone mixed with talc (a kind of *cipolin* marble); the whole exhibiting a cliff, or bare perpendicular rock, about thirty yards high. It is imbedded in the green substance, which I have mentioned as bearing considerable analogy to it, in masses that reach to the size of a few cubic decimetres [the dec. is near 4 inches], and frequently form the sides of cracks in the rock. These masses are most frequently composed of distinct pieces, and in each of these pieces the mineral is in radii diverging from a centre. Sometimes the radii are nearly parallel, and so conjoined together, as to exhibit compact masses, which divide into shapeless prisms like certain basaltes. At other times the radii, particularly when their extremities are free, terminate in true crystals. Frequently the yenite appears in long pieces, or imperfect prisms, of the size of a man's finger, and sometimes even much more slender, in the midst of the green substance; from which it is very distinct, their limits being always decidedly marked. Frequently too it is found in cavities of this substance, in crystals sometimes with a polyedral summit at each extremity, and 3 or even 4 cent. [1 inch or $1\frac{1}{2}$] long; sometimes they are solitary, at others variously grouped. The stratum that has been mentioned includes likewise epidote of a fine yellowish green, quartz, some crystals of arsenical iron, and that variety of amorphous oxidulated iron called loadstone. Its situation in the earth.

At Cape Calamite the yenite is found in the same substance,

stance, but of a grayer colour, and of an aspect similar to that of certain asbestiform actinotes. It is accompanied by oxidulated iron, garnets, and hyaline quartz*.

An old specimen said to have come from Siberia.

I have lately examined a specimen in my mineralogical collection, which I have had several years, and which, not being able to refer it to any of the known minerals, I had put into a particular place, as is my custom, for farther examination. This specimen is black yenite, imbedded and as it were disseminated in the same greenish substance. It is accompanied with a note, that marks the part of Siberia between Perm and Tobolsk for its native place. I cannot venture however to warrant the authenticity of this indication.

The substance of which I have given the history might perhaps be employed as an iron ore, and smelted for its metal, if it were more abundant than I have yet observed, and not so near one of the richest iron mines in Europe.

Always accompanied by a green mineral resembling strahlstein.

It has been seen, that the yenite, whether at Rio, at Cape Calamite, or in Siberia, is always accompanied with a green substance, disposed in fibres or rays like actinote. To this geological relation may be added, that there is a much closer in their composition: these two minerals differ only in this,

Found nine years ago.

* Mr. Fleuriau de Bellevue, to whom I showed the specimens of yenite I had, telling him I considered it as a new substance, informed me, that he himself had brought home specimens of the same mineral from Cape Calamite nine years ago, and that it was analysed by Mr. Vauquelin the year following. He at that time obtained from it

Its analysis as then given.

Silex	30
Lime	14·8
Oxide of iron	49
Oxide of manganese	2
Alumine	1
	96·8

In Romé de Lisle's collection.

Since my memoir was read, Mr. Gillet-Laumont has found in the collection of Romé de Lisle, now in his possession, crystals of the same substance; and informs me, that they were placed by that learned mineralogist at the end of the tin ores. I believe I may venture to assert, that they came from Rio. This mineral was at Paris therefore long before it was brought thither by Mr. Fleuriau de Bellevue, though it was not known.

that

that one contains a much larger proportion of iron than the other. They have besides almost all the same physical and chemical characters; whence I am induced to consider them as forming but one species. In a subsequent paper on the green substance I shall give more at large the reasons, by which I am led to this opinion; and I shall point out the place, which I conceive they ought to occupy in the classification of minerals.

XIII.

Memoir on the reciprocal Action of several Volatile Oils and certain Saline Substances: by Mr. MARGUERON, *late Apothecary-major to the Hôtel des Invalides**.

I Inserted in the 21st vol. of the Annales de Chimie a paper containing some results of the action of cold on volatile oils, and an examination of the concretions found in several of these oils. The object of the present is, to make known the reciprocal action of several saline substances with oils of that kind, and to point out the alterations such salts are capable of producing in them. Object of the author.

Exp. 1. I made a saturated solution of acetite of lead in distilled water at 15° [59° Fahr.], divided this solution equally in four phials, and added to each portion one eighth of its weight of the following volatile oils; namely those of thyme, lavender, rosemary, and sage. I shook each phial, till the oil in it was broken into globules, that the contact might be the more intimate. After keeping these mixtures several months, the following were the results. Solution of acetite of lead with volatile oils

The oil of thyme had undergone no alteration; but the part in contact with the solution contained several whitish bladders, which at the slightest motion separated in films. The oil being afterward filtered differed in no respect from what it was at first. of thyme,

The oils of rosemary, lavender, and sage, likewise expe- rosemary, la-

* Annales de Chimie, vol. XLVII, p. 46.

vender, and sage.

rienced no alteration; and no flocks formed in them, as in the oil of thyme. I filtered these oils, as I did the preceding, and assured myself, that they had not been altered by the solution of acetite of lead, which remained very clear. Into each of these oils I dropped a few drops of sulphuret of potash, which occasioned no precipitate and produced no colour.

Sulphate of alumine with oils of lavender, sage, hyssop, and rosemary.

Exp. 2. I mixed eight parts of a cold saturated solution of sulphate of alumine with one part of the volatile oils of lavender, sage, hyssop, and rosemary, each separately. These mixtures having been kept four months in flint glass phials with ground stopples, neither the oils nor the solution of alum had undergone any change.

Muriate of lime with oil of the vulnerary plants.

Exp. 3. I mixed eight parts of solution of muriate of lime with one part of the volatile oil of the vulnerary plants in a phial, and kept the mixture for a month, shaking it now and then, without perceiving the slightest alteration. In this state I added liquid potash, to decompose the calcareous muriate; but I merely found, that the oil had evidently lost some of its colour, and become whiter.

Oil of lemons and sal ammoniac.

Exp. 4. Volatile oil of lemons, expressed from the rind, being mixed with a solution of sal ammonia, and kept for a month, shaking it frequently, underwent no change.

Hyperoximuriate of potash with oils of thyme, lavender, peppermint, lemons, and cloves.

Exp. 5. Into five phials I put a solution of superoxigenized muriate of potash, made in distilled water at a temperature of about 15° [59° F.]. In one I added an eighth part of oil of thyme; in the second, an eighth of oil of lavender; in the third, an eighth of oil of peppermint; in the fourth, an eighth of oil of lemons; in the fifth, an eighth of oil of cloves. I put the bottles into a place secluded from the light, and kept them thus a month, shaking them once every day. Neither the oils nor the solution of the salt experienced any alteration. I then placed the five phials in a water bath, which I heated so as to make the water boil for a moment. These oils retained their colour, smell, fluidity, and transparency; and all of them their property of floating on water, except the oil of cloves, which sunk in it as usual. I separated each of the oils from the saline solution, evaporated each portion of the solution separately,

separately, and obtained by crystallization the superoxigenized muriate of potash just as it had been before it was dissolved.

Exp. 6. Into a phial I put two parts of recently made lime-water and one part of volatile oil of rosemary. The mixture became white on shaking it: but on standing the oil separated as fluid, and as transparent, as before, though whiter. The portion in contact with the lime-water formed a light whitish coaguium: and the lime-water had acquired a dull yellow colour, without having lost its property of being precipitable by oxalic or carbonic acid. Lime-water and oil of rosemary.

Exp. 7. Crystals of nitrate of mercury, obtained from a solution of the metal made without heat in nitric acid, were enclosed in a phial with four times their weight of oil of rosemary, and immediately a curved tube, terminating under a glass jar filled with water, was inserted into the mouth of the phial. I shook the mixture occasionally, leaving it thus for six days. I perceived no commotion, or evolution of gas; the oil acquired an amber colour; the quantity of crystals of nitrate of mercury was considerably diminished; and I observed a gray powder, among which globules of fluid mercury might be distinguished. Having poured the contents of the phial on a filter, the oil passed of a reddish colour, with the thickness of a fixed oil, and with an empyreumatic acid smell, that was predominant over that of rosemary. It contained likewise a small portion of nitrate of mercury in solution. Copper was whitened by it. Nitrate of mercury with oil of rosemary,

The mercurial matter remaining on the filter was gray, glutinous, and intermixed with globules of mercury. Spirit of wine passed through it acquired a reddish colour, and grew milky on the addition of water, like a resinous tincture. After this washing with spirit of wine a gray oxide remained, with which globules of mercury were mingled. and of lavender.

This experiment repeated with volatile oil of lavender afforded the same results.

Exp. 8. I prepared a solution of corrosive sublimate in the proportion of ten grains of the salt to an ounce of distilled water, for a series of experiments, which I shall here relate. Solution of corrosive muriate of mercury

1st. Into a phial I put one ounce of this solution and some with oil of lemons,

some volatile oil of lemons recently rectified, and perfectly colourless. I shook this mixture from time to time. In ten days the oil had subsided to the bottom in globules of a light amber colour, which, when separated by filtration, dissolved in spirit of wine. This solution being mixed with distilled water, the oil reappeared, with the property of swimming on the surface of water, being divested of the mercurial salt, which it had dissolved. The solution of corrosive sublimate separated from this oil had a somewhat acid smell, not unlike that of the residuum left after rectifying oil of lemons, and formed a yellow precipitate with lime-water as usual.

oil of chervil, 2d. Volatile oil of chervil, that had been made two years, treated with a solution of corrosive sublimate, was likewise precipitated in the form of globules, without its colour being lightened. I separated the oil in the same manner by the filter, and dissolved it in spirit of wine. Water mixed with this solution did not free the oil from the corrosive sublimate it had dissolved, for it still remained heavier than the fluid, and kept at the bottom of the vessel.

oil of hyssop, 3d. Volatile oil of hyssop, that had been long made, treated with the same solution of corrosive sublimate, sunk to the bottom of the liquid at the end of four days, without any change of colour.

oil of peppermint, 4th. Oil of peppermint, recently distilled, with the same reagent thickened, became greener, was precipitated, and adhered to the sides of the phial. The solution of this oil in spirit of wine was of an emerald green; and the addition of water caused the oil to make its reappearance with its natural green colour.

and oil of lavender. 5th. Oil of lavender, agitated in a similar solution of corrosive sublimate, was precipitated at the expiration of a few days, and became of a high amber colour approaching to red. On the sides of the phial a whitish mercurial crust was observed.

This oil, when separated from the solution, had lost its fluidity; its smell was considerably changed, being acid and empyreumatic; its colour was reddish; and it spotted glass in the manner of empyreumatic oils. Beaten up with distilled water, it first subsided to the bottom, and after some time

time rose to the surface. Shaken afterward with lime-water, it gave signs of the presence of corrosive sublimate by the yellow precipitate it formed. It dissolved completely in spirit of wine: but on the addition of water it reappeared, in part light, in part heavy. I kept some of the solution of this oil in alcohol a considerable time: and I observed, that the sides of the phial acquired a white coating, which I found to be mild muriate of mercury. The solution of corrosive sublimate separated from the oil was not entirely decomposed, it still manifesting the presence of this salt by the yellow precipitate it formed with lime water.

Exp. 9. Into a phial I put forty grains of corrosive sublimate crystallized from water, and poured on them an ounce of volatile oil of rosemary. In the course of a few days it acquired a deeper amber colour than it had before, and a white flocculent precipitate formed in it. I then added more corrosive sublimate, which changed the colour of the oil to a very deep green; the precipitate from white became green; the oil lost its fluidity; and it emitted an empyreumatic acid smell, with which that of rosemary was faintly perceptible. I separated the oil from the precipitate, mixed it with water, and heated the mixture till it boiled. The oil did not change its colour, and still remained heavy. I washed it several times with distilled water, which freed it from the mercurial salt it had dissolved; it again became light enough to swim on the surface of water; it spotted the glass in the manner of empyreumatic oils; and its solution in spirit of wine was greenish. The green precipitate mentioned above had a strong smell of rosemary, and burned with a vivid flame. On adding spirit of wine to it, the resinous part was dissolved, and formed a green tincture, which turned white on the addition of water, in the same manner as resinous tinctures. That part of the precipitate, which remained insoluble, was mild muriate of mercury mixed with a little corrosive sublimate.

Crystals of corrosive muriate of mercury with oil of rosemary.

Exp. 10. Corrosive sublimate in powder, shaken with oil of turpentine rectified from water, had no effect on this oil, though I kept the mixture a long time. The solution of this salt, on the contrary, after a certain time produced a change; causing it to assume the consistence of turpentine, while

Oil of turpentine with powdered muriate of mercury, and with its solution.

while mild muriate of mercury was deposited on the sides of the phial.

Calomel and oil of lavender.

Exp. 11. Oil of lavender kept several months on mild muriate of mercury, well washed and well levigated, underwent no alteration.

Cinnabar and oil of rosemary.

Exp. 12. Factitious cinnabar in powder and oil of rosemary exhibited no reaction during or after their mixture.

Sulphate of mercury and oil of rosemary.

Exp. 13. Oil of rosemary poured on turbith mineral, and kept some time, experienced a change. It let fall a greenish flocculent precipitate, and at the same time acquired the property of sinking in water, when poured on it. A portion of the turbith was converted into gray oxide of mercury, which retained in combination that part of the oil, that had become resinous.

Red nitrous oxide of mercury and oil of lavender, in the shade;

Exp. 14. I put into a phial a drachm of red precipitate with one ounce of volatile oil of lavender, and kept the mixture several months in the shade. The oil had formed a whitish sediment, and the red precipitate was in part reduced to gray oxide of mercury. The whitish sediment was soluble in spirit of wine, and this solution turned milky on the addition of water. The oil retained the fluidity, colour, and smell of oil of lavender, swam on water, and dissolved well in alcohol.

in the sun.

Having exposed a similar mixture to the light of the sun, the oil lost somewhat of its transparency and deposited a whitish sediment, and the red precipitate had assumed the shining metallic colour of iron filings.

Muriate of antimony and oil of rosemary equal parts.

Exp. 15. Into a small phial I put a drachm of crystallized caustic muriate of antimony, and poured on it an equal weight of oil of rosemary. On corking the phial I immediately perceived a great heat, the cork flew out, the oil spouted out with violence, and the inside of the phial remained coated with a black oil of a particular smell mixed with the smell of rosemary.

This mischance induced me to repeat the experiment in a larger vessel, and with different proportions of the ingredients.

8 parts of the oil to 1 of the muriate.

I put into a phial eight parts by weight of oil of rosemary with one of crystallized caustic muriate of antimony. The heat was scarcely perceptible; the muriate of antimony fell

fell to pieces very slowly; and the portion of oil that was in contact with it acquired a deep amber colour. In a few days the whole of the oil was turbid, and exhibited interiorly a flocculent precipitate. As soon as the oil appeared to be grown clear, I poured it out on a filter, on which a light orange coloured matter remained, that I shall notice presently. The oil passed through of a deep amber colour; it had the consistency of an expressed oil; and its smell was less sweet. Being kept a long while in a phial, it deposited a whitish sediment. Poured into distilled water, and gently shaken, it separated into flakes, which rose to the surface of the water, and let fall a silky precipitate, of a very white and silvery appearance. This was insoluble both in water and in alcohol, was not volatilized at a red heat, and resembled the silvery flowers of antimony. The oil dissolved easily in alcohol, separated from it on the addition of water, and let fall on standing a precipitate similar to that abovementioned. The orange coloured matter left on the filter, having been washed repeatedly with spirit of wine, and dried on paper, exhibited a crystalline appearance in certain parts. Put on a slip of iron, and heated to redness, it became covered with shining needles, as muriate of antimony does.

Exp. 16. Oil of rosemary added to a solution of nitrate of silver exhibited after some time a whitish pellicle, which had a metallic aspect, and formed a separation between the solution and the oil. The oil did not appear to be altered, and the solution of nitrate of silver still formed muriate of silver on the addition of muriatic acid. The pellicle was a portion of the silver reduced to the metallic state. — Nitrate of silver and oil of rosemary.

Exp. 17. The volatile oils in the experiments lately mentioned having undergone a great deal of alteration, I thought it right, to continue the examination of the same oils with a less active substance, as sugar, with which they are supposed to be capable of forming a perfect combination, completely soluble in water, without losing any thing of their properties. Such a compound is called an *oleosaccharum*, and is used to impart to different preparations, solid or liquid, the smell of a fruit, flower, or some other part of a vegetable. — Volatile oils with sugar.

vegetable. Whatever be the proportion of sugar and volatile oil directed in different Dispensatories, the authors are agreed on the intimate nature of the union; and conceive, that this method renders volatile oils miscible with aqueous liquors.

For an oleosaccharum the oil should be highly rectified.

Volatile oils reduced to an oleosaccharum are the more easy to unite with aqueous liquors, in proportion as the oils are more fluid, and more highly rectified. When they are so to a sufficient degree, they even dissolve in a large quantity of water alone. Mr. Baumé remarks, in his Elements of Pharmacy, that essential oils, when they quit a vegetable to rise in distillation with water, undergo even in this a true rectification. That which rises first with the milky water is more fluid and more fragrant, than what passes after the receiver has been changed. The latter does not whiten the water: the former dissolves in it in consequence of its tenuity, and gives it that milky whiteness, which is observed as long as it passes over in the distillation. Accordingly an oleosaccharum made with these first oils is agreeably aromatic, does not render the aqueous liquor turbid, and does not separate from it: while on the contrary the second, of which we have been speaking, or any other volatile oil that has been made a certain time, or a mixture of both these oils recently made, forms an oleosaccharum that is not of a pleasing smell or taste, renders the aqueous mixture turbid, and separates from it in a very short time. We may ascertain this fact, by putting into a glass tube filled with water an oleosaccharum made with this oil. It will fall to the bottom; the air contained in the sugar will be extricated; the oil will rise through the fluid in small globules and collect on the surface; if the mixture be shaken it will turn milky, and be a long while becoming clear again; but the oil will at length reappear, having experienced a sort of thickening, so that instead of globules it will form a sort of mucilaginous flakes. The oleosaccharum then is an imperfect combination, when it is prepared with an oil not sufficiently rectified. It is to be observed, that many lozenges made by baking, and flavoured with essential oils, as those of peppermint, anise, &c., are oleosaccharums

The best oil rises first.

The combination of the oil with the sugar

saccharums that do not let the oil separate on solution in water, though commonly the makers are indifferent about employing highly rectified oils; not to mention, that the most volatile part of the oil must be dissipated by the heat, to which it is exposed during the baking of the sugar. It would seem, that in this case the heat must produce a more intimate combination with the oil, the nature of which it would be interesting to know. promoted by baking.

Exp. 18. As I had at hand a certain number of essential oils, I thought it not amiss to make a series of experiments, for the purpose of ascertaining the heat or cold they would produce when shaken with water. These experiments furnished me with a new mode of detecting those adulterated by a mixture of spirit of wine. Experiments to find whether heat or cold be produced on mixing oils with water.

1. I mixed volatile oil of peppermint with thrice its weight of distilled water, and plunged a mercurial thermometer into the mixture. The temperature was 10° [50° F.], and no change took place. Oil of peppermint. No change.

2. Common oil of peppermint of the shops, mixed with water in the same proportion, at the same temperature raised the thermometer $1\frac{1}{2}°$ [2·7°]. Common oil of peppermint. Heat.

3. Oil of lemons recently rectified from water on a water bath, as limpid and as fluid as ether, being shaken with distilled water, produced neither heat nor cold. Oil of lemons.

4. Oil of orange flowers recently distilled comported itself in the same manner; while the orange flower oil of the shops with the same quantity of water raised the thermometer 1° [1·8°]. Oil of orange flowers.

These experiments having taught me, that some essential oils produce neither heat nor cold by their mixture with water; while others on the contrary produce a sensible heat, though operating with very small quantities; I conceived, that the cause of this heat was ascribable to spirit of wine, employed to adulterate them. Accordingly I mixed one part of spirit of wine with two of an essential oil, kept the mixture some time, and then mixed it with thrice its weight of water, when it caused the thermometer to rise 1° [1·8°]. When heat is produced, they are adulterated with spirit of wine.

General

General results.

General conclusions.

From the experiments above related it follows:

Effects of acetate of lead, or of alum;

1. That the oils of thyme, lavender, rosemary, sage, and lemons, undergo no alteration, even by standing, with solution of acetate of lead, or of alum.

of muriate of lime;

2. That the oil of the vulnerary plants with a solution of muriate of lime, loses its lemon yellow colour, and becomes whiter.

of hyperoxi-muriate of potash;

3. That the solution of superoxigenized muriate of potash occasions no change in the oils of thyme, lavender, peppermint, lemons, or cloves.

of lime water;

4. That lime-water destroys in part the colour of oil of rosemary.

of nitrate of mercury;

5. That nitrate of mercury is decomposed in oil of rosemary, which it renders high coloured.

of corrosive sublimate;

6. That corrosive sublimate, and its solution in distilled water, increase the colour and consistency of the oils of lemons, chervil, hyssop, lavender, and rosemary, and are partly decomposed by them, producing mild muriate of mercury.

of calomel and cinnabar;

7. That mild muriate of mercury and factitious cinnabar occasion neither action nor reaction with oils of lavender and rosemary.

of turbith mineral;

8. That turbith mineral is partly decomposed in oil of rosemary.

of red precipitate;

9. That red precipitated mercury is in part converted into gray oxide in oil of lavender, without causing the oil to undergo the least alteration.

of muriate of antimony;

10. That caustic muriate of antimony is decomposed in oil of rosemary, which it colours and thickens; while part of this muriate loses its acid, and appears to be converted into silvery flowers of antimony.

of sugar;

11. That an oleosaccharum is a more or less perfect combination, according to the oil employed.

and of water.

12. Finally, that volatile oils shaken in distilled water produce no heat, that is sensible to the thermometer, unless they have been adulterated with spirit of wine.

XIV.

XIV.

Analysis of the Schist that accompanies the Menilite near Paris: by Prof. LAMPADIUS*.

THIS mineral was formerly confounded with *polierschiefer*, or polishing slate: but Werner has given it the name of *klebschiefer*, or adhesive slate, on account of its property of adhering strongly to the tongue. On his return from France he gave me a certain quantity, that I might subject it to chemical examination. Adhesive and polishing slate formerly confounded.

The following are its external characters, as given by Werner. It adheres strongly to the tongue. Its colour is of a pale yellowish gray. It is without lustre. Its fracture is slaty, with straight leaves. It is opake. Scratching gives it a little lustre. It is very tender. It separates into leaves spontaneously, which is one of its principal characters. It is light, not being twice as heavy as water. Characters of adhesive slate.

It serves as a gangue to the menilite, with which it is found in the hill of Menil-Montant near Paris. Where found.

The following are the results of my chemical experiments on it. Analysis.

a. Roasted for two hours in a very active wind furnace it lost 30 per cent of its weight. Its colour became a deep brown. It exhibited no signs of fusion, either in a clay crucible, or in a crucible lined with charcoal: yet it had become harder, and less friable. That which had been roasted in the clay crucible was rendered very attractable by the magnet. Calcined.

b. Before the blowpipe on charcoal and with oxigen gas it melted in a few seconds into an opake glassy bead, of a blackish brown colour. Treated with the blowpipe and oxigen gas:

c. Exposed to the flame of the blowpipe simply, it was not possible to melt it: but with borax a small portion was dissolved, and coloured of a blackish brown. with borax.

These preliminary trials, and its effervescence with muriatic acid, led me to suspect, that it contained carbonic acid and iron.

* Journal de Mines, N. 106, p. 317. Extracted from a work published by prof Lampadius in 1804 under the title of *Beytrage zur Erweiterung der Chemie.*

d. A

Roasted. *d.* A thousand parts of the mineral roasted in a retort yielded 270 of carbonic acid.

Dissolved in muriatic acid. *e.* Another thousand parts dissolved in ten times their weight of muriatic acid lost 270 parts.

It contains therefore 27 per cent of carbonic acid.

I afterward proceeded with the analysis in the following manner.

Treated with sulphuric acid. 1. One part of the mineral was well powdered, and put into four parts of concentrated sulphuric acid, in which it dissolved with evident effervescence; and the solution was evaporated to dryness.

Silex. 2. The residuum was diffused in water, and a gelatinous matter separated, which was still a little yellowish. This was silex.

3. The liquor was filtered.

4. The gelatinous residuum was washed with boiling water, till no farther trace of sulphuric acid was discoverable.

5. This water and the filtered liquor were evaporated together, till there remained but ten drachms.

Lime. 6. Some sulphate of lime separated, which was decomposed by an alkaline carbonate; and after it had been heated and roasted 0·08 of pure lime were obtained.

Iron and magnesia. 7. The liquor separated from the sulphate of lime was concentrated by heat, and it yielded crystals of sulphate of iron, and of sulphate of magnesia.

8. Without separating the crystals I put the whole into a platina crucible, and exposed the saline mass to a strong heat for two hours.

9. After cooling, the mass had an ochry colour, and a bitter taste. On it I affused boiling water, filtered and washed the residuum.

Iron. 10. The oxide of iron remained on the filter. After having been dried and roasted it weighed 0·09.

Magnesia. 11. I added to the liquor carbonate of ammonia, when a white earth was precipitated, which dried and roasted appeared to be magnesia, and weighed 0·28.

12. The yellowish gelatinous residuum (No. 4) was digested in muriatic acid, till its colour became entirely white.

More iron. 13. Being filtered and washed, the liquor was of the colour

lour of pale white wine. Being precipitated with ammonia, I obtained some more oxide of iron, which washed and roasted weighed 0·03.

14. After having redissolved this oxide of iron, and that of No. 10, there yet remained 0·008 of silex. Silex to be deducted from the iron.

15. The residuum of No. 13 was found to be pure silex, which, after having been dried and roasted, weighed 0·30. Silex.

Accordingly 100 parts of this mineral contain. Component parts.

Magnesia	28
Carbonic acid	27
Silex	30·8
Oxide of iron	11·2
Lime	0·8
Water	0·3
	98·1
Loss	1·9
	100

The most remarkable circumstance is, that this mineral contains no alumine, and includes a large quantity of iron. The outward appearance of the mass would lead us to suspect the former substance, and its light colour by no means indicates so large a quantity of the second. Probably the carbonic acid combining with the oxide of iron conceals its presence*. Contains no alumine and more iron than expected.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

AT the last meeting of the Wernerian Natural History Society (14th May), Mr. P. Walker read an account of the Wernerian Society.

* Mr. Klaproth, who had before analysed a specimen of this schist, found in it: Klaproth's analysis of it.

Silex	66·5
Alumine	7
Magnesia	1·5
Lime	1·25
Oxide of iron	2·5
Water	19
	97·75
Loss	2·25
	100

birds

birds that frequent the vicinity of Edinburgh. He enumerated 178 species; of which 11 belong to the genus falco, 4 to strix, 1 to lanius, 8 to corvus, 1 oriolus, 1 cuculus, 1 picus, 1 alcedo, 1 upupa, 1 certhia, 2 sturnus, 6 turdus, 1 ampelis, 2 loxia, 7 emberiza, 8 fringilla, 1 muscicapa, 3 alauda, 15 motacilla, 4 parus, 4 hirundo, 1 caprimulgus, 2 columba, 1 phasianus, 6 tetrao, 3 ardea, 6 scolopax, 7 tringa, 4 charadrius, 1 hæmatopus, 3 rallus, 3 fulica, 4 podiceps, 4 alca, 6 colymbus, 2 sterna, 12 larus, 1 procellaria, 5 merganser, 20 anas, 4 pelecanus. This account was accompanied with interesting observations on the distinctions of several of the species, their changes of plumage at different ages and times of the year, and their kind of food; and specimens of some of the dubious species were exhibited.

Mr. Jameson, at the same meeting, read the following mineralogical queries, and stated the reasons, that induced him to consider the objects pointed out by them as deserving the particular attention of mineralogists.

Mineralogical queries.

Mineralogical queries.

1. In what species of rock are the metalliferous veins of Tyndrum situated, and what are the minerals they contain?

2. Are the leadglance veins of Strontian situated in sienite, and what are their geognostic relations?

3. Are the trap-veins, that traverse the mining field at Strontian, basalt, porphyry slate, or greenstone, or do all these different species of rock occur in that district?

4. Does the quartz-rock of Scuraben and Morven in Caithness, and of Portsoy in Bamffshire, occur in an unconformable and overlying position, or does it belong to the comformable primitive rocks, as clay slate or mica slate?

5. Does not the granular rock of Ben Nevis rather belong to the sienite than to the granite formation?

6. Does the rock of the hill of Kinnoul near Perth belong to the flœtz-trap or newest flœtz-trap formation?

7. Is the mountain of Cairnsmuir in Galloway composed of old granite?

8. What

8. What is the extent and particular geognostic relations of the black pitchstone of Eshdale Muir in Dumfries-shire?

9. Does the black pitchstone of the Cheviot hills belong to the newest flœtz-trap formation?

10. On what formation does the porphyry slate of Braid Hills near Edinburgh rest, and what are the venigenous and imbedded fossils it contains?

11. What are the geognostic characters and relations of the edge and flat coal beds or seams in Mid Lothian?

12. On what formation does the Calton Hill near Edinburgh rest?

13. Does the greenstone of Corstorphine Hill belong to the independent coal formation?

14. Does the hill on which the town of Stirling is built belong to the coal formation?

15. What are the geognostic characters and relations of the veins that traverse or are included in the greenstone of the independent coal formation?

16. What are the characters of the transition greenstone of the south of Scotland?

17. What are the particular species of petrifactions that occur in the transition limestone near the Crook, on the road from Edinburgh to Moffat?

Mr. Parkinson's second volume of organic remains of a former world is intended to be published in June. It will contain twenty plates, representing the figures of nearly two hundred different fossils of the remains of zoophytes, coloured after nature; among which are the mineralized remains of upwards of twenty species of the encrinus. It cannot but be gratifying to our readers to know, that of these remains the greater number have been found in different parts of this island. Parkinson's organic remains.

Dr. Satterley and Dr. Young propose to give two courses of Medical Lectures next winter at the Middlesex Hospital. Dr. Satterley's will be Clinical lectures, and any of the pupils of the hospital attending them will have the privilege of seeing the patients whose cases are discussed. He will be assisted in the department of morbid anatomy by Mr. Cartwright. Dr Young's course will be on the Elements of the Medical Sciences in general, and on the Practice of Physic in particular. It has been erroneously stated in several periodical publications, that Dr. Young had a large medical work nearly ready for the press; the errour arose from his having been for some time engaged in the preparation of these lectures. Medical lectures.

METEOROLOGICAL JOURNAL

For MAY, 1808,

Kept by ROBERT BANKS, Mathematical Instrument Maker, in the STRAND, LONDON.

N. B. The apparatus and its relative situations will be described in our next.

MAY. Day of	THERMOMETER. 11 A. M.	11 P. M.	Highest.	Lowest.	BAROMETER.	WEATHER. Night.	Day.
2	57	58	62	56	29,93	Cloudy	Fair
3	60	61	60	52	—,85	Moonlight	Do. Rain at
4	61	60	76	56	—,81	Ditto	Do. [9 A.M.
5	62	60	68	56	—,83	Ditto	Do.
6	62	61	70	56	—,77	Ditto	Do.
7	64	60	69	52	—,62	Rain	Rain
8	55	53	58	48	—,57	Cloudy	Fair
9	49	48	56	45	—,54	Moonlight	Ditto
10	54	49	59	47	—,79	Ditto	Ditto
11	54	50	60	51	—,95	Ditto	Ditto
12	58	62	56	54	30,17	Cloudy	Ditto
13	61	61	67	59	—,26	Ditto	Ditto
14	68	63	74	61	—,18	Ditto	Ditto
15	70	72	77	66	—,16	Fair	Ditto
16	74	69	80	61	30,00	Ditto	Ditto
17	68	64	74	56	—, 8	Cloudy	Ditto
18	58	51	59	44	—,11	Ditto	Rain
19	54	50	57	46	—,22	Fair	Fair
20	57	55	65	54	30,00	Cloudy	Ditto
21	57	55	58	54	29,76	Ditto	Rain
22	58	57	59	54	—,50	Ditto	Ditto
23	58	54	63	49	—,66	Fair	Ditto
24	59	57	65	49	—,84	Cloudy	Fair
25	58	62	66	50	—,93	Ditto	Ditto
26	60	59	60	53	—,73	Ditto	Rain
27	59	56	61	51	29,74	Fair	Ditto
28	61	55	64	53	30,00	Ditto	Fair
29	61	60	64	56	30,18	Ditto	Ditto

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JULY, 1808.

ARTICLE I.

An Essay on Polygonal Numbers, containing the Demonstration of a Proposition respecting Whole Numbers in general. In a Letter from JOHN GOUGH, *Esq.*

SIR, *Middleshaw, May* 26, 1808.

THE design of the present essay is, to demonstrate the following general propositions: every whole number is either a polygonal number of a given denomination *m*; or it may be divided into polygons of that denomination, the number of which does not exceed *m*. This singular property of numbers was discovered by Mr. de Fermat, who I believe gave it to the world without a demonstration. Should the present attempt to supply the defect obtain your approbation, the publication of it will oblige — Proposition to be demonstrated.

Yours, &c.

JOHN GOUGH.

Definitions. 1st. The sum of any arithmetical progression, beginning with unity, is called a polygonal number, and a polygon at times in the present essay. — Definitions.

2d. The difference of two adjacent terms in the generating series increased by two is called the denomination of the polygonal number; and the number is said to be digonal, trigonal, tetragonal, &c., accordingly as the denomination $m = 2, 3, 4,$ &c.

3d. The number of terms a, which are added together to form any polygonal number, is called its index; and the polygon is said to be of the first, second, or third order, &c., accordingly as $a = 1, 2, 3,$ &c.

Prop. 1. *Proposition* 1*st*. *Lemma*. Let b be the greater of two adjacent terms, $a\ b$, in a series of digonal or natural numbers; then we have $\frac{b^2-b}{2} = \frac{a^2-a}{2} + b - 1$. For $b = a + 1$, and $b - 1 = a$; but $\frac{b^2-b}{2} = \overline{b-1} \times \frac{b}{2} = a \times \frac{a+1}{2} = \frac{a^2+a}{2}$: again $\frac{a^2-a}{2} + b - 1 = \frac{a^2-a}{2} + a = \frac{a^2+a}{2} = \frac{b^2-b}{2}$, Q E D.

Corollary. *Corollary*. Hence if we put $a = 1, 2, 3,$ &c. successively, each succeeding value of $\frac{a^2-a}{2}$ may be found, by adding the next value of a in succession to that of $\frac{a^2-a}{2}$ last found, and subtracting unity from the sum; where it is evident, that when $a = 1$, $\frac{a^2-a}{2} = 0$. In this manner the annexed table is constructed, the use of which will appear in the sequel.

Table.

$a =$	1	2	3	4	5	6	7	8	9	10	11
$\frac{a^2-a}{2} =$	0	1	3	6	10	15	21	28	36	45	55

Prop. 2. *Proposition* 2*d*. If k be a polygonal number, of which the denomination $= m$; and index $= a$, we have $k = a + \overline{m-2} \times \overline{a^2-a}$. For the first term of the generating series $= 1$ (by *Def.* 1st); and the common difference of the terms $= m - 2$ (by *Def.* 2d); but the term of which the number

number is a, $=1+\overline{m-2}\times\overline{a-1}$ (Emerson's proportion, prop. 6) $=\overline{m-2}\times a+3-m$; moreover the sum of a terms of the series $=k$ (by *Def.* 1) $=\frac{a}{2}+\overline{m-2}\times\frac{a^2}{2}+\overline{3-m}\times\frac{a}{2}$ (proportion prop. 7); therefore $k=a+\overline{m-2}\times\overline{\frac{a^2-a}{2}}$, Q E D.

Cor. 1*st.* Every polygon of the first order $=1$; and every one of the second order $=m$; this is proved by substituting 1 and 2 for a in the expression $a+\overline{m-2}\times\overline{\frac{a^2-a}{2}}=k$. Cor. 1.

Cor. 2*d.* All polygons of higher orders may be found by the table in prop. 1st, thus: to the given index a, add the product of the corresponding number $\frac{a^2-a}{2}$, multiplied by $m-2$, and the sum is the polygon. Cor. 2.

Example.—Thus the sixth decagon $=6+15\times 8=126$.

Prop. 3*d.* Let g, h, l, &c. be polygonal numbers of a common denomination m, the indices of which are b, c, d, &c. respectively; also put $b+c+d$, &c. $=a$, and let k be the polygon of which the denomination $=m$ and index $=a$; then $g+h+l$, &c. $+\overline{m-2}\times\overline{bc+bd+cd}$, &c. $=k$. For by addition and prop. 2, $g+h+l$, &c. $=b+c+d$, &c. $+\overline{m-2}\times\overline{\frac{b^2+c^2+d^2}{2}}-(\overline{m-2})\times\overline{\frac{b+c+d}{2}}$; but $\frac{b^2+c^2+d^2}{2}=\frac{a^2}{2}-(bc+bd+cd)$ by involution; hence by substitution $g+h+l=a+\overline{m-2}\times\overline{\frac{a^2-a}{2}}-\overline{m-2}\times\overline{bc+bd+cd}$; therefore $g+h+l+\overline{m-2}\times\overline{bc+bd+cd}=a+\overline{m-2}\times\overline{\frac{a^2-a}{2}}=k$ (by prop. 2) Q E D. Prop. 3.

Scholium. Put $m=4$, and we have (by def. 2 and prop. 2) $g=b^2$, $h=c^2$, $l=d^2$; therefore $g+h+l+\overline{m-2}\times$ Scholium.

$\overline{bc+bd+cd}=b^2+c^2+d^2+2\times\overline{bc+bd+cd}=k=a^2$.

Theorem confined to squares here generalized.

Thus it appears, that the present proposition generalizes a theorem formerly confined to squares, and extends it to polygons of all denominations.

Prop. 4.

Prop. 4th. Put e and v respectively equal to $g+h+l$, &c., and $bc+bd+cd$ in prop. 3; then $v=\frac{a^2-a}{2}+\frac{a-e}{m-2}$

For $e+\overline{m-2}\times v=k$ (by prop. 3) $=a+\overline{m-2}\times\frac{a^2-a}{2}$ (by prop. 2); hence $v=\frac{a^2-a}{2}+\frac{a-e}{m-2}$. Q E D.

Cor. 1.

Cor. 1st. Since b, c, d, &c., are integers, v is an integer; but $\frac{a^2-a}{2}$ is an integer, therefore $\frac{e-a}{m-2}$ is an integer; hence if $\frac{e}{m-2}$ give a quotient s and a remainder p, $a=p$ or $\frac{a}{m-2}$ gives a quotient r and a remainder p; from which we have the following general expressions $e=p+\overline{m-2}\times s$; $a=p+\overline{m-2}\times r$; $v=\frac{a^2-a}{2}+r-s$.

Cor. 2.

Cor. 2d. Put $p=0, 1, 2, 3 \ldots m-3$ successively, then e will be expressed as in the following table.

$p=$	0	$e=$	$0=m-2 \quad =2m-4 \quad =3m-6$, &c.
$p=$	1	$e=$	$1=m-2+1=2m-4+1=3m-6+1$, &c.
$p=$	2	$e=$	$2=m-2+2=2m-4+2=3m-6+2$, &c.
$p=$	3	$e=$	$3=m-2+3=2m-4+3=3m-6+3$, &c.

It appears from the table, that the values of e, taken vertically, constitute the series of natural numbers; therefore every integer is either a polygon of a given denomination m, or it may be resolved into polygons of that denomination.

Prop. 5.

Prop. 5th. $b^2+c^2+d^2$, &c. $=a+2s-2r$. For $b^2+c^2+d^2+2v=a^2$ by involution; but $2v=a^2-a+2r-2s$ (cor. 1, prop. 4); therefore $b^2+c^2+d^2=a+2s-2r$. Q E D.

Cor. 1.

Cor. 1st. $b^2-b+c^2-c+d^2-d=2s-2r$, because $b+c+d=a$.

Cor.

Cor. 2*d.* Since a is any term of an arithmetical progression, bounded by p and e, and having $m-2$ for its common difference, prop. 4. cor. 1; it will be easily understood, that r is also a corresponding term of an arithmetical progression, of which the extremes are o and s, and common difference 1; hence it follows, that $2s-2r$ increases, while r and a diminish; therefore $b^2-b+c^2-c+d^2-d$ increases, while the sum of the roots $b+c+d$ diminishes; consequently, the number of the parts b, c, d, into which a is divided, decreases at the same time. Cor. 2.

Cor. 3*d.* If a can be so taken that $a+2s-2r=a^2$, $b=a$; and e is a polygon to the index a and denomination m. Cor. 3.

Prop. 6*th.* If e be an aggregate of polygons of any denomination m, and y that polygon, which is less than e, but greater than any polygon of an inferior order and the same denomination; then the polygons, into which e can be resolved, are equal to y or less than y. For the next superior polygon is greater than y (by prop. 2); it is therefore greater than e by hypothesis, and cannot constitute a part of it. Q E D. Prop. 6.

Cor. If $e=y+m-1$, it may be resolved into m polygons of the denomination m; namely, into y and $m-1$ units; again, if $e=y+m$ it may be resolved into polygons, the number of which is less than $m+1$; this is evident from cor. 2, prop. 4: lastly, if e, $=y+t$, can be resolved into polygons, the number of which $=m-f$, $e+f$ may be resolved into m polygons of the same denomination. It is evident, from the properties of polygonal numbers contained in this corollary, that every whole number may be resolved into polygons of the denomination m, the number of which does not exceed m. Cor.

Prop. 7. *Problem.* To resolve a given number e into polygons of a given denomination m, by help of the table in prop. 1. Prop. 7.

Method. 1st, Write down all the successive values of a, beginning with e and diminishing them progressively by $m-2$, until the series terminates with 0, or with p less than $m-2$; 2d, under each value of a place the corresponding

sponding value of $s-r$, making the series begin with 0, and increase by unity, until it ends with s; 3d, these preparatory steps being completed, take any value of $s-r$ in the work, and find what numbers in the second column of the table will produce the same when added together; then if the indices of these numbers, when added in like manner, give the index of $s-r$ in the work, they are also the indices of the constituent polygons; but if the sum of the numbers taken from the table prove $=$ to the given value of $s-r$, while the sum of their indices is less than the corresponding value of a in the work, the deficiency may be made up in the latter sum by the addition of units, because one is the index of 0 in the table.

Example 1. *Example* 1st. Resolve 14 into pentagons. According to the directions given above, the work will stand thus:

$$e = 14;\ a = 14 \,.\, 11 \,.\, 8 \,.\, 5 \,.\, 2$$
$$s-r = 0 \,.\, 1 \,.\, 2 \,.\, 3 \,.\, 4$$

Here the first value of $s-r=0$ resolves e into 14 units, because 0 in the table has 1 for its index; and $0 \times 14 = 0 = s-r$, and $1 \times 14 = 14 = a$. The second value of $s-r = 1$ resolves 14 into 9 pentagons of the first, and 1 of the second order, for 1 in the table has 2 for its index, denoting a polygon of the second order; but $2+9=11=a$ in the work. The third value of $s-r=2$ resolves 14 into 4 pentagons of the first and 2 of the second order, for 2×1 in the table $=2$, the double of the index of which $=4$ and $4+1 \times 4 = 8 = a$. The fourth value of $s-r=3$ resolves 14 into 3 pentagons, namely, into 2 of the first and 1 of the third order, for 3 in the table has 3 for its index, and $3+1 \times 2 = 5 = a$. The last value of $s-r=4$ will not resolve 14, because $3+1=4$ and the sum of their indices $=3+2=5$, which is greater than 2 or a in the work.

Example 2. *Example* 2d. To resolve all the numbers from 16 to 24 into tetragons or squares, which shall not in any case exceed 4 in number. It is evident from cor. 2, prop. 5, and the last example, that all the values of a may be rejected in the present instance, which are greater than the index of 16, namely 4, in the table to prop. 1, when this number

number is considered separately; consequently, the corresponding values of a in all the remaining numbers, 17, 18 ... 24 may also be rejected, and the collateral values of $s-r$ placed under the last series; which being done, the work will stand thus.

$e = 16$;	$a = 4$	. 2	. 0				
$e = 17$;	$a = 5$	. 3	. 1				
$e = 18$;	$a = 6$	. 4	. 2	. 0			
$e = 19$;	$a = 7$	. 5	. 3	. 1			
$e = 20$;	$a = 8$	. 6	. 4	. 2	. 0		
$e = 21$;	$a = 9$	. 7	. 5	. 3	. 1		
$e = 22$;	$a = 10$	. 8	. 6	. 4	. 2	. 0	
$e = 23$;	$a = 11$	. 9	. 7	. 5	. 3	. 1	
$e = 24$;	$a = 12$	. 10	. 8	. 6	. 4	. 2	. 0
$r-s =$	6	. 7	. 8	. 9	. 10	. 11	. 12

Here the first index of $16 = 4$, and $s-r=6$; therefore $16 =$ a square of the fourth order by table; first index of $17 = 5 = 4 + 1$; $s-r = 6 = 6 + 0$; first index of $18 = 6 = 4 + 1 + 1$; $s-r = 6 + 0 + 0$: first index of $19 = 7 = 4 + 1 + 1 + 1$; $s-r = 6 + 0 + 0 + 0$; hence 19 is resolved into 4 squares, 18 into 3, and 17 into 2: again, second index of $20 = 6 = 4 + 2$, $s-r = 7 = 6 + 1$; therefore, 20 is resolved into two squares, namely, one of the second and one of the fourth order: second index of $21 = 7 = 4 + 2 + 1$, $s-r = 7 = 6 + 1 + 0$; second index of $22 = 8 = 4 + 2 + 1 + 1$, $s-r = 7 = 6 + 1 + 0 + 0$; therefore 22 is resolved into 4 squares, and 21 into 3: again, second index of $23 = 9 = 3 + 3 + 2 + 1$, $s-r = 7 = 3 + 3 + 1 + 0$; that is, 23 is resolved into 4 squares: lastly, third index of $24 = 8 = 4 + 2 + 2$; $s-r = 8 = 6 + 1 + 1$; that is, 24 is resolved into 3 squares.

In the same manner any other number may be resolved into polygons of any denomination m, so that the number of these polygons shall not exceed m, denoting the denomination.

II.

II.

On a Variety of the Brassica Napus, or Rape, which has long been cultivated upon the Continent. By Mr. James Dickson, *F. L. S. V. P. H. S**.

Great improvement of wild vegetables by culture.

IN the report drawn up by our worthy member, T.A. Knight, Esq., at the request of a Committee of this Society, and printed by their orders, it is remarked, that nature appears to have put no limits to the success of our labours in improving vegetables, if properly applied: that thus our wild crab has been converted into the golden pippin, and that our most delicious plums have originally sprung from the sloe. The vegetable which I have now the honour of laying upon your table, gentlemen, is one more among many instances of the truth of the above remark; which I have quoted, because I think it cannot be too frequently repeated, and instilled into the minds of young gardeners. Nature has undoubtedly done much in furnishing our table with a variety of esculents spontaneously, but when we aid her efforts to befriend us, by industry on our part, she, like a kind mother, never disappoints us. Who would suppose, that the hard acrid root of the brassica napus, or common rape, might be rendered so mild and palatable by cultivation, as to be preferred to the common turnip? yet this has actually been the case, and in France as well as Germany few great dinners are served up without it in one shape or other.

Synonimes.

I am unable to trace its first coming into such common use there; but, as it is distinguished by Gaspar Bauhin, who published his Pinax in 1671, it must have been well known at that period. The only synonyms I dare put down as certainly belonging to it are, brassica napus, *β. Linn. Sp. Pl, ed.* 2, p. 931; napus sativa, *C. Bauh. Pin.* p. 95; le navet, *Gallis*; Teltow rüben, *Germanis*; French turnip, *Anglis*.

Use of them.

For above twelve years I have seen this plant brought to our own market in Covent Garden, but only by one perfon: and I believe it has been chiefly sold to foreigners, though, when

* Transact. of the Horticultural Society, Part I, p. 26.

when once known, it will be a very acceptable root in most families. It is much more delicate in flavour than our common turnip, and is to be used in the same way. In Germany, it enriches all their soups, and there is no necessity to cut away the outer skin, or rind, which is thinner than that of the common turnip, but only to scrape it. Stewed in gravy, it forms a most excellent dish, and, being white, and of the shape of a carrot, when mixed alternately with those roots upon a dish, is very ornamental. The following different receipts for dressing them are by an eminent French cook.

Dish prepared from them.

"Take your roots, and wash them very clean with a brush; then scrape them, cutting a thin slice away from the top, and as much from the bottom as will make them all of equal lengths: boil them in water, with a little salt, till they are tender; then put them into a stewpan, with a gill of veal gravy, two tea spoonfuls of lemon pickle, one of mushroom ketchup, a little mace, and salt, and let them just simmer, but not boil, for a quarter of an hour; thicken the gravy with flour and butter, and serve them up hot."

Another.

"Take your roots, and after preparing and boiling them as before, put them into a stewpan, with a little water, working in as much flour and butter as will make it as thick as cream; let them simmer five minutes, then place the stewpan near the stove to keep hot: just before you dish them, add two large spoonfuls of cream, mixed with the yolk of an egg, and a little mace beat very fine, shaking the pan over the fire for two or three minutes, but do not let them boil. Put white sippets of French bread round the dish."

A third.

"Take your largest roots, clean them as before, and cut them in slices as thick as a crown piece; then fry them till they are of a pale brown colour on both sides; after which, put them into a stewpan, with as much water as will cover them, to simmer for ten minutes; then add a large spoonful of Madeira or Xeres wine, the same of browning, a few blades of mace shred, two tea spoonfuls of lemon pickle; thicken the liquor with a little flour and butter, and serve them up with toasted sippets round the dish."

Requires no manure and is best in a poor sandy soil.

One great advantage attending the cultivation of this vegetable is, that it requires no manure whatever; any soil that

that is poor and light, especially if sandy, suits it, where it seldom exceeds the size of one's thumb or middle finger; in rich manured earth it grows much larger, but is not so sweet or good in quality. The season for sowing the principal crop is any time from the middle of July to the end of August, or even later in this country, where our frost seldom sets in before Christmas. If the season should prove dry, it will be necessary to water the beds regularly, till the plants have got three or four leaves, otherwise they will be destroyed by the fly; and this crop will supply the table till April. If wanted during the whole year, a little seed may be sown the latter end of October, and these plants, if they do not miscarry, will be fit for use in April or May. The last crop may be sown from the middle of January to the middle of February, which will also come in the end of May and June, but in July and August they will not be very good, and as at that season of the year there is an abundance of other vegetables, it is of less consequence; upon a north border, however, and in a sandy moist soil, it is possible to have them sweet and tender during the whole summer.

Mode of culture.

Saving the seed.

To save good seeds, you should, in February, or the beginning of March, transplant some of the finest roots, placing them two feet asunder, and keeping the ground repeatedly hoed: when the seedpods are formed, they should be guarded from the birds, either with nets, or shooting some, and hanging them up upon sticks. As soon as they change colour, cut the heads, and spread them to dry in the sun, after which beat out the seed, and lay it up for use.

III.

Comparative Analysis of some Varieties of Steatite, or Talc; by Mr. VAUQUELIN*.

Unctuosity of steatites supposed owing to magnesia.

IT has commonly been supposed, that the smoothness and unctuosity of the stones called steatites were owing to the presence of magnesia, because this earth had been found in every one analysed, and in consequence all the stones that had these

* Annales de Chimie, vol. XLIX, p. 74.

external

external characters were classed together. But the pierre de lard, or speckstein, which in some respect may be considered as the prototype of the species, having been analysed by Klaproth, and no magnesia found in it, has changed the opinions of mineralogists on this subject, and led them to wish, that some of these substances should be analysed anew. Bildstein contains none.

With a view to remove this uncertainty, Mr. Haüy gave me three varieties of talc, that I might make a comparative analysis of them. Three varieties analysed:

The first of these is termed in Haüy's Mineralogy laminar talc. It is of a greenish white colour when seen in the mass, very smooth to the touch, and divides into exceedingly thin, flexible laminæ of a silvery white. laminar talc;

The second is called in the same work talc glaphique, because it is employed in sculpture; but commonly pierre de lard. It is the bildstein of the Germans*. This is compact, very greasy to the touch, and of a colour varying between gray, yellowish, and greenish. Its fracture is dull, uneven, and at the same time scaly. bildstein.

Of this species Mr. Haüy sent me two specimens; one of a yellowish white, from a broken Chinese image; and the other of a light rose colour, but in every other respect perfectly similar to the preceding specimen.

Analysis of the flexible laminar talc.

Laminar talc

1. I calcined in a strong fire a hundred parts of this stone. By this operation it acquired a yellow colour, with a light rosy tint, was deprived of its flexibility, and lost six parts of its weight. Its laminæ being thus rendered very fragile, I could easily reduce it to powder. calcined;

2. The hundred parts thus calcined I heated with twice their weight of caustic potash. The mixture did not melt; but its tumefaction indicated, that a combination between the substances had taken place. heated with potash;

3. The mixture diluted with water was afterward dissolved in muriatic acid, and evaporated to dryness in a gentle heat. Toward the end of the operation the liquor formed a jelly. treated with muriatic acid;

4. The residuum, being lixiviated with distilled water, lixiviated;

* Agalmatolite of Klaproth, pagodite of Napione, steatite pagodite of Brongniart. Tr.

left

left a white powder, which when calcined in a red heat weighed 62 parts. It was pure silex.

precipitated with ammonia,

5. Ammonia, mixed with the liquor separated from the silex, formed in it a yellow precipitate of little bulk, from which a part and half of alumine were separated by means of caustic potash. The remainder was oxide of iron, weighing three parts and half.

and boiled with carbonate of soda.

6. After having precipitated the iron and alumine by means of ammonia, I put into the liquor a solution of carbonate of soda, and set it to boil. As soon as the mixture began to grow hot, it grew turbid and deposited a large quantity of a white powder, which when washed and calcined weighed 27 parts. This substance was magnesia, for with sulphuric acid it formed a salt, that had all the characteristics of common sulphate of magnesia.

Flexible laminar talc therefore is compounded of

Results of the analysis.

Silex	62
Magnesia	27
Oxide of iron	3·5
Alumine	1·5
Water	6
	100

Considering the smallness of the quantity of the iron and alumine, I think these substances may be presumed not to be essential to the formation of the stone; so that perfectly pure laminar talc may be deemed a compound of silex and magnesia.

Analysis of compact rose-coloured talc.

Compact rose-coloured talc.

In the analysis of this variety I pursued the same processes as in that of the preceding; I therefore need not enter into the particulars, but the following are its results.

Results of its analysis.

Silex	64
Magnesia	22
Alumine	3
Iron mixed with magnesia	5
Water	6
	100

Analysis

Analysis of the yellowish compact talc (speckstein.) Bildstein

1. A hundred parts of this stone strongly calcined lost 5 parts. calcined;

2. Heated afterward with twice its weight of potash in a silver crucible no fusion took place, but the matter was greatly increased in bulk, and had become homogeneous. heated with potash;

3. This was diffused in water, and dissolved in muriatic acid. The solution, being evaporated, became gelatinous toward the end of the operation. treated with muriatic acid;

4. The matter being dried, and washed, a white powder remained, which, after calcination, weigned 56 parts. lixiviated;

5. The silex having been separated by lixiviation, the liquor was mixed with a small quantity of muriatic acid, and ammonia was afterward poured in, which formed in it a copious white flocculent precipitate. precipitated with ammonia;

6. The liquor being filtered, the precipitate was washed and dried. This was alumine, and weighed 30 parts. The alumine dissolved entirely in sulphuric acid, and its solution, saturated with the requisite quantity of potash, afforded very pure alum: but the mother water, evaporated afresh, yielded $5\frac{1}{2}$ parts of sulphate of lime crystallized in needles. Thus with the assistance of the alumine the ammonia precipitated the lime from its solution in muriatic acid. treated with sulphuric acid;

7. The liquor from which the alumine had been separated gave no precipitate with carbonate of soda, even assisted by long boiling. The speckstein therefore contains no magnesia, like the two preceding varieties. and carbonate of soda, but nothing thrown down.

But in recapitulating the products of this analysis we find only 93 parts; namely Results of this analysis.

Silex	56
Alumine	29
Lime	2
Iron	1
Water	5
	93

A loss so considerable, which is not common in such analyses carefully executed, led me to suspect, that the Loss too great.

compact

compact talc contained some other principle, which the processes employed did not enable me to discover. In consequence I treated a hundred parts, reduced to fine powder, with concentrated sulphuric acid.

Treated with sulphuric acid,

1. After boiling for two hours I dried the mixture, lixiviated the residuum with distilled water, and boiled the lixivium. At the expiration of a few days I obtained 36 parts of alum crystallized in cubes: and by a second evaporation I procured from the mother water 15 parts more of the same salt, mixed with a few needly crystals of sulphate of lime.

Cubic crystals of alum obtained.

2. As the stone appeared to me but imperfectly decomposed, I powdered it afresh, and treated it as before. On adding the acid employed in this operation to the mother water of the preceding, I obtained 15 parts more of alum, making in all 60 parts. Then, as I employed for this operation very pure sulphuric acid, and added no potash to the solution, it is evident, that the stone contained a certain portion of this alkali, and that this substance was the occasion of the loss I had in the first analysis.

Treated with fresh sulphuric acid,

and more alum produced.

Sixty parts of alum however do not require seven of potash, the quantity of loss; but as the stone is very siliceous, it is probable, that the whole of the potash was not extracted by the sulphuric acid, though I boiled the stone twice in it.

The whole of the potash probably not extracted.

The speckstein therefore is composed of

Its true component parts.

Silex	56
Alumine	29
Lime	2
Iron	1
Water	5
Potash	7
	100

Mr. Klaproth, in his analysis of speckstein, found no potash: but the quantity of water, which according to him amounts to 10 per cent, and the loss of $2\frac{1}{2}$, which he experienced, will just balance the deficiency I found. It is probable, that Mr. Klaproth estimated the water by computation,

Klaproth reckons too much water.

putation, and not by direct experiment; for, to whatever heat I exposed the stone, it never lost more than 5 per cent.

From the analyses here given it follows, that of the three varieties of talc here mentioned, two only must continue to be so called; namely the laminar talc, and the compact rose-coloured talc. The third, the speckstein, should be removed to the genus of *alkaliniferous* stones. Bildstein therefore an alkaline stone.

It is particularly remarkable, that those two varieties, which most resemble each other, and which have always been classed together, should now be separated by analysis: which shows, that minerals should never be classed according to their external appearance, since the most striking analogies in this respect are the most deceitful. Minerals should not be classed by their external characters.

In fact, the speckstein and compact rose-coloured talc have the same softness, the same fineness of particles, the same fracture, nearly the same specific gravity; and certainly, if there were any room to suppose, that one of the three substances ought to be separated from the talc species, we should be more inclined to suppose it the laminar, than either of the others.

Note. On this occasion I analysed that species of talc known by the name of *craie de Briançon*, or French chalk, and I found it to contain the same principles, and nearly in the same proportions, as the laminar talc, and the compact rose-coloured talc. These proportions were, Analysis of French chalk.

Silex	61·25
Magnesia	26·25
Water	6
Alumine	1
Oxide of iron	1
Lime	0·75
Loss	3·75
	100

IV.

Observations on the Crystallized Substances included in Lavas: by G. A. Deluc*.

Various conjectures respecting the causes and effects of volcanoes.

VOLCANOES occupy such a striking place among terrestrial phenomena, that they have become the subject of numerous conjectures respecting their origin, their influence, and the geological consequences deducible from them.

Have been applied too generally to geology.

Wherever natural philosophers or geologists have imagined they might be called in to found a system, they have made them act whatever part appeared most suitable to their purpose; so that from a simple and solitary fact, single of its kind, and influencing only the ground occupied by the volcano and its vicinity; and although the volcano resembles only mountains of its own kind, and in no respect other mountains, either in shape, construction, or component parts; they have nevertheless concluded, that the strata and mountains on the surface of the Earth owe their origin to the action of fire: fire, say they, daily exhibiting to us productions identical with the primitive rocks of our globe.

Crystals in lava supposed to be formed in it.

Hence it is, that these naturalists consider the different crystals included in lava, not as products in the humid way anterior to the lava, that existed in the strata which the volcanic fires brought into a state of fusion, but as crystallizations formed in the lava itself, and from its substance, by the slow refrigeration of the mass.

This the foundation of Fl. de Bellevue's system.

On this opinion chiefly is founded the system, which Mr. Fleuriau de Bellevue has adopted, and lately published†, respecting the action of the fire of volcanoes, and the formation of the terrestrial globe, its strata, and its mountains.

Simple statement of the question.

This question, reduced to its most simple terms, is this: have the crystals included in lava been formed in the lava, and from its substance; or are they foreign to it, and formed anteriorly, in the humid way, in the strata or substances which the volcanic fire reduced to the state of fusion? And

* Journal des Mines, No. 115, p. 5.

† Journal de Physique, May, 1805.

an

an examination of this question, deduced from the true state of things, and carried by facts to a degree of evidence that excludes all doubt, would decide one of the most important points in geology, by exhibiting a just idea of volcanoes and their phenomena.

The principal argument of Mr. Fleuriau de Bellevue is drawn from the analogy he finds between the formation of the crystals contained in lava, and that kind of crystallization, which has been called *crystallites*, and is formed in the pots in glass-houses, when the glass that was in fusion is suffered to cool slowly.

Crystals of glass chief foundation of the system.

Let us now examine what these crystallites of glass-houses are. The whole mass of cooled glass exhibits a confused crystallization, all of the same tint, in which we see small compact bars confusedly interlaced, some slightly striated, and others disposed in the form of stars, equally confused. At other times a number of threads are formed at the bottom of the pot, which cross and intermingle with each other, and exhibit likewise stellate figures.

These crystals described.

In the first case these crystallites compose the substance of the glass, and are distinguished only in some places. In the second we see at the bottom, through the transparent glass, these bundles of nets, and starry figures, which have some resemblance in shape to the little icy stars, that fall with snow in very cold weather. Perhaps some instances of more decided vitreous crystallizations may occur: but these, which are rare, only prove, that there may be some circumstance favouring this crystallization in a very small space.

Mr. Fleurieu de Bellevue conceives, that these crystallite figures *singularly resemble* tremolite. This opinion, that there exists a striking resemblance between two substances, one of which is the product of a glass-house, the other of a mineral stratum, appears to me astonishing I confess; for in this way there is no substance, which we may not reckon similar to another, provided they have some similarity in form. Thus we may say, that the capillary schoerls, or mineral byssus, resemble hairs; and that the fibres of the amianthus, or stone-flax, resemble those of flax or hemp; though these substances merely resemble each other in form, without there being any real similarity between them.

These said to resemble tremolite:

but they have merely some similarity in shape.

This remark was necessary, as it might be supposed, from the expression *singularly resemble*, that it was something more than in appearance, and this not very close.

Tremolite described.

The tremolite, which derives its name from the vale of Tremola near St. Gothard, one of the principal places in which it is found, is a radiated mineral substance, the threads of which, most commonly of a shining white, are united in sheaves or bundles. These bundles issue from a common centre, and diverge around it, which gives them the figure of a radiated star; and these centres being various, they give different directions to the radii, which are from half an inch in length to three inches or more. This mineral substance is one of the most curious and pleasing to the eye. It is sometimes intermixed with talc and calcareous spar; that is with two substances, one of which is vitrifiable but of difficult fusion, and the other calcinable: a circumstance of itself sufficient to exclude the least resemblance between tremolite and the products of glass-houses. And if we compare these products with the slender and brilliant threads of the tremolite, each of which taken separately has the form of a quadrilateral prism, we shall be surprised, that they were ever compared with each other. The tremolite is vitrifiable; but it is not and never was vitrified.

Cannot have been crystallized from fusion.

Crystals in lavas.

Let us now turn our attention to the crystallized substances included in lavas, to which the vitreous crystallites have been compared. This comparison I am able to make on a great number of pieces which I have collected from burning and from extinct volcanoes.

Several kinds of them, different from each other, and from the lava.

The lavas that include leucites, or white garnets, frequently include likewise volcanic schoerls, augites or pyroxenes*, and chrysolites or olivines. Here are three species of crystals, very distinct from each other both in figure and colour, contained in the same lava; enveloped in the same paste, which has no resemblance to either of them in nature, colour, or chemical properties, as will soon appear.

* I shall mention these in future by the name of pyroxene schoerls, because the denomination of pyroxene does not belong to them exclusively, all the substances contained in lavas being equally pyroxene, or strangers to fire.

The

The form of the leucites and volcanic schoerls is perfectly determinate; there is nothing in it confused, but all is precise and well marked. The leucite is constantly of a round figure, cut with twenty-four trapezoid faces, and of a gray white colour. The volcanic or pyroxene schoerl is an octaedral prism with two diedral pyramids, of a deep olive colour, and sometimes black. The chrysolite has its peridot colour, and its three crystals are found in the cellular and spongy lava, as well as in the compact.

Their figure well marked, and colour retained.

The schoerl is strongly adherent to the lava, so that it cannot be detached, and appear with its faces polished and angles entire, but by a chemical operation, the effect of the sulphurous acid fumes of the volcano. The leucite is more easily separable, leaving impressed on the lava its round form, with the shapes of its facets as clearly marked as they are on the leucite itself. Its impressions in the lava may be compared to those left by garnets, cubic martial pyrites, and several other crystallized substances, on the rocks that include them; with this difference, that the impressions of the leucite were made on a substance in fusion, and those of the garnet and pyrites on a rock that was soft from humidity.

The schoerl united to the lava.

Leucites more easily separated, leaving their impression on the lava.

Hence we may draw this inference, that the leucites were no more produced in the lava at the time of its cooling, than the garnets and pyrites were formed from the substance of the rock, which encloses them now it is dried and hardened. Both are equally foreign to the matter that contains them, and existed before it; the leucites before the lava, and the garnets and pyrites before the rock in which they are imbedded. Leucites are also found separate, and in great numbers, among volcanic ashes.

Formed before it therefore, as the crystals found in rocks.

In this exact statement of facts, can we perceive any resemblance, any analogy, between the crystallised substances included in lava, and those confused heaps of vitreous crystallites formed of the substance of the glass in the pots in glass-houses? or between those fantastic forms of cooled glass, and the crystals in the strata of our mountains, all of a constant and regular form, each in its kind?

No analogy between these and the crystals of glass.

The pyroxene schoerls too are found separate, and sometimes in multitudes innumerable. The crater that opened in

Loose pyroxenes in great numbers on Etna:

in the base of Etna in 1669, which raised a cone 4300 paces in circumference at its base, whence issued the enormous lava that we see in existence, and the bulk of which astonishes us, exhibits a singularly striking example. The summit of this crater is covered with these schoerls mixed with small scoriæ; and this remarkable circumstance attends them, the schoerls on the outside of the crater have all without exception retained on their surface a crust of the lava that included them, while those within exhibit their native polish.

Those without the crater encrusted with lava; those within, not.

This accounted for by the action of the volcanic fumes on the crust.

I will here explain the cause of this difference, and I believe I am the first who have attended to it. The sulphurous acid fumes of the volcano penetrate and decompose the surface of the lavas and scoriæ, that are exposed to it; and the schoerls, which these fumes do not attack, then appear in relief, and entire throughout, being perfectly cleaned from the lava that surrounded them; as rock crystals, when they are covered, as they are sometimes, with a calcareous tufa, are freed from it by nitric acid, and appear with all their brilliancy. This operation proves, that there is no chemical affinity between the lava and the pyroxene schoerl it includes, since the one is attacked and dissolved, and the other is not. The effect of this is sometimes a pleasing sight, as it exhibits schoerls of all sizes, even microscopic, fixed on the lava, the surface of which has been decomposed, shining with their native polish and their angles perfectly sharp.

Sometimes the pyroxenes themselves attacked.

It sometimes happens, that the schoerls themselves are attacked, and their colour altered to such a degree, as to appear like small crystals of sulphur, or still whiter. This effect is produced, no doubt, when the fumes contain a mixture of acids, that act on schoerl when combined, which they could not do separately: a chemical operation of which we have a well-known instance in the aqua regia, composed of the nitric and muriatic acids.

In the volcanic ashes at Pompeii a leucite united to a schoerl.

To these facts, which evidently prove, that these crystallized substances were anterior and foreign to the lava including them, I shall add as a superabundant proof a singularity, found among the ashes that covered Pompeii, and now in my collection of volcanic substances. This is a solitary

litary leucite of three or four lines diameter throughout its whole crystallization, united to a schoerl, the greater part of the prism of which it embraces. This schoerl too is perfectly crystallized, and each of these crystals retains its proper colour. It appears by some vestiges still adhering to the schoerl, that these two crystals were enveloped in a reddish spongy lava.

A leucite enclosing 3 schoerls.

This is not the only singularity I possess. I have another that came from the same place, though not so well defined, because it has retained more of the lava. This too is a leucite of the same size, perfectly distinct, and including a small groupe of schoerls, one of which is larger than the other two united with it.

Similar to rock formations.

Are not these instances similar to those that frequently occur to crystals of strata formed in the humid way? to those green schoerls, or epidotes, we see included in rock crystals; those micas, those pyrites, included in the same kind of crystal; and this in its turn enveloped in crystals of calcareous spar: unions that indicate a succession of formations. The green schoerls, micas, and pyrites, have preceded the rock crystal; and the rock crystal the calcareous spar. We find also combinations of these three crystals including each other in the same order, whence this natural consequence follows, that the pyroxene schoerl preceded the leucite in formation.

Leucites in the fissures of lavas.

I would likewise remark, that spongy lavas exhibit in their fissures leucites in part isolated, the greater part solitary, but some in groups, as happens with crystals of all kinds. Is this the course, is this the appearance, of these confused heaps of crystallites of glass cooled in the glass-houses?

No leucites in the lavas of Etna; and no crystalline laminæ in those of Vesuvius: yet both contain pyroxenes and chrysolites.

We are not acquainted with any lava of Etna that contains leucites; or any of Vesuvius that encloses those whitish crystalline laminæ, which are so abundant in the lavas of Etna. This is a fact, to which the naturalist, that supposes these crystals to be formed in the lava, ought to pay some attention. If the leucites were really formed in it, why do the lavas of Etna contain none, while they are filled with pyroxene schoerls and chrysolites, which they possess in common with the lavas of Vesuvius? Is not this

difference

difference accounted for much more naturally by the absence of leucite in those strata, from which the lavas of Etna proceeded?

Lavas of Hecla

The same variations are observed in the lavas of different volcanoes. Those of Hecla, of which I have considerable specimens brought home by Sir Joseph Banks, contain neither pyroxene schoerls, nor leucites, nor chrysolites, but a great many small, white, cracked, crystalline substances, from the size of a grain of hempseed to that of a pea, of an irregular figure, having the appearance and hardness of quartz, of which they appear to be fragments. The lavas of Mont-d'Or, an ancient volcano of Auvergne, contain large crystals of amphibole, or hornblende, and feldspar, which show by their cracks and vitreous reflections, that they have experienced the action of incandescent lava; and in other ancient volcanoes in Auvergne we find pyroxene schoerls without leucites.

and Mont-d'Or.

Leucites in the lake of Andernach.

The small gravel of the volcanic lake of Andernach is filled with loose pyroxene schoerls, whole and in pieces. Should we find in this state the confused radii of cooled glass, which formed part of the mass, and could only be separated from it in shapeless fragments?

Eruption of Vesuvius in 1754.

Among the facts I adduced against the opinion of Sir James Hall, quoted by Mr. Fl. de Bellevue, whose opinion is the same, I mentioned a singular eruption of Vesuvius, that happened in 1754. A mouth opened nearly at the level of the valley, which separates the present cone from Mount Somma. At the rise of the lava this mouth formed a grotto, which it lined by its spirtings with a quantity of scoriæ in a stalactitical form, the intertwined jets of which are from three to six lines in diameter, of a reddish colour, and full of blebs. In the fragments of these jets I have found pyroxene schoerls in a state of perfect crystallization, and with their deep olive colour. These spirtings indicate, that the lava was in a high degree of fusion, and such slender jets must have cooled and hardened the moment they were separated.

Grotto lined with the stalactitical scoriæ

containing pyroxenes.

No slow cooling here.

We have here no slow refrigeration to form crystals, nor mass sufficient to give rise to crystalline forms by this mean: yet we find in these jets pyroxene schoerls, and for the

the most part even on their surface. Is not this a farther proof, that these crystals were preexistent to the lava? Mr. Fl. de Bellevue does not admit this conclusion: yet, if we attend to the fact, it will be found very convincing. The surface of the jets of this singular stalactite, and that of the interior of the blebs, are covered with a multitude of shining points, which are perceptible only by the reflection of the light. When viewed by a lens with a high magnifying power, they appear to be very slender bundles of sublimed iron.

Sublimed iron in minute particles on these jets.

I will mention another remarkable fact, the discovery of which required all the attention, with which I have examined volcanic phenomena.

The branches that separate from a stream of lava, or the lava itself when it is not abundant, break into fragments at their extremity, which in this case have no progressive motion, but by the flowing of these fragments pushed forward or to the sides by an impulse from the interior. These fragments heaped up retain their heat a long time. This is seen when they are viewed by night; and felt in the day by the heat they diffuse, as well as by the sulphurous and mephitic gasses they exhale. These fragments broken off from the lava itself, which have not ceased for a moment to be red hot, exhibit at their surface pyroxene schoerls. I possess two such fragments, which I took from the spot myself, and which have several. What can reasonably be objected to so many facts? Yet I abridge the enumeration of them.

When lava breaks into fragments, before it is cooled below redness, pyroxenes are seen in it.

"Mr. Salmon and Mr. de Buch," says Mr. Fl. de Bellevue, "have demonstrated, to all those who are acquainted with existing volcanoes, that the crystals of leucite could only have been formed during the slow refrigeration of the lava."

Opinion of Salmon and de Buch.

I am acquainted with existing volcanoes, of which I have just given proofs; yet from my observations I draw a quite opposite conclusion. The facts I have cited, which are true and exact, decide the question.

Contradicted.

With regard to the opinion of these two naturalists I will add, that it sins in an essential point. What ground is there for distinguishing the leucites from the pyroxene schoerls

Why should leucite be formed in lava more than the other crystal

schoerls and chrysolites, since these three crystals are found together in the same lava, separated from each other, and from the matter of the lava, by a line as clear and distinct as separates the small pebbles, that compose a pudding-stone, from the cement that envelopes them? If one of these crystals be foreign to the lava, so are the other two: this is a natural consequence. The fact assuredly is, that they are all three foreign to it.

Argument against their igneous formation.

The two instances I have mentioned of isolated leucites enveloping pyroxene schoerls are inexplicable facts, on the hypothesis of these crystals having been formed in the igneous way: while nothing is more common, or more easy to conceive, than such combinations between crystals of different kinds formed in the humid way.

Patrin's new Dict. of Natural History.

"I should never have done," says Mr. Fl. de Bellevue, "if I were to bring forward all the objections, that offer themselves to the system of the preexistence of crystals in lava. Several will be found at the articles Lavas and Leucites, in the new Dictionary of Natural History, in which Mr. Patrin has strongly combated these suppositions."

We should regard facts, not suppositions.

I am sorry to learn this, since the readers of that Dictionary, who are desirous of knowing what lavas and leucites are, will be led into errour. I have exhibited *facts*, and not *suppositions*. In the physical phenomena of our globe, the accurate knowledge of which depends always on matters of fact, I never was fond of suppositions, which seldom fail to lead us into some mistake.

In the lava of Viterbo leucites calcined.

Let me remind Mr. Fleuriau de Bellevue of a very remarkable lava of the ancient volcanic mountain of Viterbo. This lava contains a multitude of leucites from the size of a large pea to that of a rapeseed. These leucites have undergone a kind of calcination, which renders them very white, and the lava that includes them is black, which occasions a striking contrast between these substances. Nothing more strongly marked can be conceived. Now is it not evident, that all these leucites existed before the lava? If we dispute this conclusion, we may as well deny, that any foreign substance whatever, included in a rock, has existed before the rock.

Leucites acted

Leucite does not resist the action of volcanic fires and vapours

vapours in the same degree as schoerl; as their effects upon it appear to be not inferior to those upon the lava. At least none of the pieces I possess, that have been exposed to their action, afford any leucite in good preservation; though it retains its characteristic form in the midst of red hot lava. When the heat is carried higher, it is capable of softening it, and occasioning it to undergo a sort of calcination. It then cracks, and the matter of the lava penetrates these cracks in the leucite; whence we perceive within it particles of lava, which are distinguishable by their black or brown colour, and little blebs: but the form of the leucite is preserved, because, as the lava entirely surrounds it, no part of its surface can separate from it. This is the case with the leucites of the ancient lava of Viterbo; and on the piece in my possession there are several niches of leucites, with the impression of their facets. The lava and leucites coming together out of the fires of the volcano, as the lava there must be more perfectly fused than when it flows exposed to the open air, and in its subterranean course must meet with narrow passages in which it is compressed, its matter must penetrate more easily into the cracks of the leucites.

upon by volcanic fumes more easily than schoerl.

It has been said, that no leucites are found in lavas that have flowed with rapidity, but that they are confined to such as have flowed slowly. This is a mere ideal distinction: for by what signs can we determine, whether a lava have flowed slowly or rapidly? I fancy it would puzzle any man, to determine this with certainty: and besides, what change can the less or greater velocity or slowness of its course occasion in the substance of a lava?

Not easy to determine whether a lava have flowed slowly.

The following is a very remarkable fact related by Mr. Dolomieu. "Loose leucites are so abundant in the vicinity of Rome, that the road from Rome to Frescati may be said to be covered with them. The rain washes them away, and collects them in vast quantities in the ditches by the roadside." To this fact Mr. Dolomieu subjoins some conjectures respecting the origin and formation of the leucites, in which I think he is mistaken, though he is far from supposing them to have been formed of the matter of lavas.

Loose leucites abound near Rome.

I have not seen this singular place, but I possess a pretty

They come

large

from decomposed lava.

large number of these very leucites, from the smallest size to that of a little cherry. They must have come from spongy lava at no great distance, that has been decomposed. I have seen some of the same nature near Civita Castellana; the whole surface of which was spotted with a multitude of white grains. Unfortunately, and to my great regret, it rained very hard at the time, which prevented my alighting from my carriage. How can we conceive, that the multitude of loose leucites at Frescati were formed from the substance of the lava that included them? They are a little transparent, and of a slightly yellow colour: is there in this any analogy with the colour or matter of lava? Indeed we might as well maintain, that the garnets included in a rock have been formed of the substance of that rock.

Similar ones near Civita Castellana.

Crystals said to be formed in the crater.

Mr. Fl. de Bellevue imagines, that the crystals thrown out separately by the crater "are new products, formed in the crater itself by a first cooling."

None but by sublimation.

Nothing is formed in the crater, or to speak more accurately on its sides, but crystals of salts and sulphur by sublimation; and never any crystal of solid matter like those contained in lava.

The lava said to cool in the crater, before its eruption.

To support this opinion he fixes two epochs: the first of which, according to him, takes place in the crater itself. A first cooling in the crater! But let us admit this supposition. Thus we have a lava cooled and hardened: but from a lava come to this state none of the substances contained in it could be separated so as to appear loose: for this it must be plunged again into the fire of the volcano; and would it not there enter again into fusion?

The crystals merely thrown out by the explosions.

The crystals that are found detached on the cone of a crater have been separated in the crucible of the volcano itself by the ebullition of the melted lava, and the jets of its explosions. The crater that opened on Etna in 1669 exhibits a very instructive example. The very large cone raised by this opening is covered with an innumerable multitude of pyroxene schoerls, all without exception covered by a slight crust of the lava that contained them, mixed among the small scoriæ in which some are included. This lava cannot have been cooled for a moment from the first instant

Crater on Etna of 1669.

instant of its fusion; yet we here find a multitude of crystals, that issued from the crater ready formed. Can these have been produced by a first cooling of the lava? The enormous mass of this lava, that issued from the foot of the cone, contains itself a prodigious quantity of these schoerls: all their edges are distinguishable on the surface of the fractures.

Another fact respecting this lava.

This same lava, and the jets of its explosions, exhibit another interesting fact. It includes, beside the pyroxene schoerls, a multitude of small crystalline laminæ of a whitish colour, that have no regular form, and appear to be scales from some substance splintered by the heat. These laminæ are found detached likewise, mingled with the schoerls and little scoriæ. Can we discover here that *play of affinities*, to which the formation of the crystals included in lava is ascribed, since here is no regular form? Besides, the play of affinities can take place only when the molecules, on which they act, are at liberty to unite, which they cannot be, except in a mass perfectly fluid: and this is not the state of lava, in which it is asserted to occur. They are in fusion, no doubt; but it is a dull, heavy fusion, that has no progressive motion but on steep descents, or from the successive impulses given by the matter that issues from the volcano, and pushes before it, while at the same time it spreads at the sides, that which preceded it. How can affinities be exerted in such a mass?

Cannot arise from the action of affinity.

Matters thrown up by the explosions contain pyroxenes

The burning matters thrown up by the explosions of the crater, some of which are drops of compact lava, others fragments torn from the mass in fusion, contain likewise pyroxene schoerls, which show themselves entire, when these fragments have been exposed to the corrosive action of the vapours of the crater. This action is sometimes carried so far, as to reduce these fragments to a degree of softness little less than that of dough: and the schoerls there being in perfect preservation, they are well distinguished by their black colour on the yellow sulphurous tint of this paste, which acquires some consistency in drying, but is easily broken. I have collected several pieces in these different states, which are now before me. We cannot suppose, that there was a moment of first cooling in this case: since

since these pieces were thrown from the focus of the volcano, at the very moment when its contents were in the highest fusion.

The products of volcanoes should be compared with those of art.

"One of the most natural ideas, that present themselves, to solve so many difficulties," says Mr. Fl. de Bellevue at the outset, "must be carefully to compare the products of volcanoes, and the circumstances in which they are found, with the results of those large bodies of fire, by means of which man separates, dissolves, brings together, and combines minerals, and produces in them a change of form." This I have just done. I have compared the products of our glass-house furnaces with those included in lava, and the result of my comparison is, that they are totally different.

This done.

(To be concluded in our next.)

V.

Experiments on Molybdena: by CHRISTIAN FREDERIC BUCHOLZ.

(Continued from p. 138.)

VI. *Phenomena presented by molybdena exposed to the action of fire in contact with atmospheric air.*

Molybdena calcined.

Exp. 22. A Piece of molybdena in the metallic state, weighing fifty-three grains, of a moderate consistency, and an ashen gray colour, was put into a Hessian crucible, and the heat raised gradually. Scarcely had the heat reached a deep red, when the surface of the metal became of a brownish yellow, and soon changed to a fine violet, inclining to indigo. The metal being withdrawn from the fire and broken, its central part was still gray, and had undergone no alteration. From this nucleus to the surface the colour proceeded in gradation through a yellow and brownish yellow to blue. The metal having been again exposed to the same degree of fire for a sufficient time, it became entirely blue;

Oxided in different degrees.

blue: but many precautions were necessary to attain this result, because the surface passed very readily to a higher degree of oxidation, and quickly reddened. On this blue mass I poured cold water, which partly dissolved it; and by boiling I completed the solution, which was equally of a blue colour.

Blue oxide.

Dissolved in water.

When the crucible, in heating it more strongly, passed to a deep red, the metal quickly began to burn, putting on likewise a deep red appearance. At this degree of heat it kept its deep blue colour. The fire being increased, the metal was brought nearly to a white heat, and after cooling, its surface, to the depth of a few lines, was of a blueish white; nearer the centre it was of a blue inclining to violet; and the nucleus was violet inclining to brown, like the matter obtained in decomposing molybdate of ammonia by heat. The metallic mass, which had little consistency till the action of the fire had given its surface a white colour, became more compact and tenacious, so that it was difficult to crumble it between the fingers. On urging the fire the whole surface became enveloped by the molybdic acid that was formed; and this acid gradually increased in quantity, till at length it entered into fusion.

Exposed to a greater heat.

Acid formed.

These phenomena evidently indicate different degrees of oxidation. The brownish oxide may be considered as the first degree. The violet brown oxide is very probably at the same degree of oxidation, as that obtained by exposing the molybdate of ammonia to a red heat. The blue oxide soluble in water seems to contain a larger quantity of oxigen; while the blueish white oxide may be considered as a mixture of the blue oxide with white oxide, the last of which is probably nothing but molybdic acid, that fuses and sublimes at a higher heat. Thus these different oxides may be arranged in the following order: the light brown, the violet brown or violet, the blue, and the white.

Different degrees of oxidation.

Their order.

Of these oxides the blue chiefly attracted my attention, more particularly on account of the different manners, in which it may be produced by oxidation and disoxidation, in the treatment of molybdena by acids, alkaline sulphurets, metallic solutions, &c.

Blue chiefly noticed.

Exp.

Experiments on the blue oxide of molybdena.

Experiments on this.

Exp. 23. Fifty grains of metallic molybdena powdered were put into a porcelain crucible, placed in a sloping direction on the fire, and heated till the surface acquired a blue colour. On first heating the powder became of a brownish yellow, which soon changed to a copper brown. This colour remained some minutes, till the crucible acquired a greater heat. The metal burned in a part where the crucible scarcely began to be of a dull red. Immediately on this I drew back the crucible, and kept it for a quarter of an hour exposed to a moderate heat, constantly stirring the powder. The brown colour thus changed completely to a grayish blue, and the powder carefully collected and weighed had gained an addition of five grains, or one tenth. Having poured on it an ounce of water, and shaken it a few minutes, a very small portion only was dissolved. On keeping the mixture for two hours at a heat of 30° [86° F.] the solution assumed a deep sapphire blue colour, and a bitter metallic taste. Having decanted the solution, and poured a fresh quantity of water on the residuum, I proceeded as before, and obtained a very pale blue solution. The residuum I boiled with two ounces of distilled water in a china cup till half the fluid was wasted; and when the powder had subsided, I had a fine deep sapphire blue solution. The same thing took place on repeating this process.

Not so soluble as in the preceding experiment.

Thus the oxide formed in this experiment did not comport itself like that obtained in the preceding (*Exp.* 22.) where the blue oxide obtained by the calcination of metallic molybdena dissolved in water completely. In this present case the blue oxide appears to have penetrated the rest of the mass, and prevented the whole from being oxided to this point, by which it had become itself less soluble.

Residuum.

The residuum when dried weighed twenty grains, and was of a dark gray inclining to brown, which led me to believe it was a mixture of brown oxide and metal. I then put it again into the cup, and roasted it cautiously; and in fact, as soon as I began to heat it, its colour changed to brown inclining to blue, till by degrees it became entirely blue. After having boiled it three different times with two ounces of

Heated again.

of water till half was evaporated, I obtained a blue solution. Still I had a residuum of fifteen grains, which was of a copper brown inclining to blue. This I set aside for the present, and made a trial with a large quantity of metal, in order to find a readier method of obtaining the blue oxide. Still a residuum.

Exp. 24. I reduced two hundred grains of metallic molybdena to as fine a powder as possible, and treated this as in the preceding experiment. A copper brown oxide was formed, which became blue on continuing the heat. When it was nearly of an indigo blue, with a tint of gray, and began to burn in different places, I withdrew it from the fire, put it into two ounces of water, and boiled it till half was evaporated. A blue solution was thus obtained, and the residuum was treated three times in the same way. The last residuum had entirely lost its blueness, and acquired a copper colour: however, I boiled it thrice more, and the solution was still blue. This is an evident proof, that simple boiling in water changes the brown oxide into blue oxide, and consequently that the latter is more oxided. Exp. 24. Boiling changes the brown oxide to blue.

I now attempted actually to convert the brown oxide that remained into blue oxide by continued ebullition in water, and for this purpose I put it into a large vessel with sixteen ounces of distilled water, which I boiled till it was reduced to two. The solution was blue it is true, but not to such a degree as I expected from so long boiling. I therefore tried whether the brown residuum would not be more easily changed into blue oxide, if I merely moistened it and afterward dried it repeatedly. This I did ten times; and each time I poured an ounce of water on the residuum, which I boiled for five minutes. The solution was still blue, and in this way I reduced the brown oxide to eleven grains. Attempts to effect this completely.

This mode of preparing the blue oxide is very troublesome, I was sensible of the defect, and I sought by several methods to find a better. I had observed, that, when a solution of molybdena in sulphuric acid is decomposed by an alkaline sulphuret, and that afterward a little sulphuric acid is added, the precipitate, that was formed in the first instance, is decomposed, and a blue solution is produced. Trials to produce the blue oxide with less trouble. With sulphuric acid.

But

Could not be collected on account of its solubility.

But I could discover no method of collecting the blue oxide in its pure state; for, after I had evaporated the solution of this oxide, I could not separate the residuum, on account of its great solubility, either from the sulphuric acid, or from the alkaline sulphate formed by means of the sulphuretted alkali. A portion of sulphur too remained in this residuum. It is true the alkalis separated a small quantity of oxide, when the solution was concentrated, but its solubility did not permit me to wash what was on the filter. I must observe too, that an excess of alkali destroyed the blue colour; consequently it is probable, that it occasioned a higher degree of oxidation.

Blue colour destroyed by an alkali.

Other acids turn the molybdic blue.

Muriatic acid.

To effect the separation sought, I endeavoured to avail myself of the experiment of Scheele and other chemists, namely, that molybdic acid, when dissolved in other acids, affords a blue liquor. The muriatic acid appeared to me most proper, on account of its volatility. Accordingly I dissolved two drachms of brown oxide, obtained by calcining molybdate of ammonia, in moderately concentrated muriatic acid. The solution during ebullition changed from brownish yellow to yellowish green, and lastly to a deep blue. I evaporated to dryness, and obtained a mass of a dull blue, but I could not free it entirely from the acid that adhered to it. On washing it, it was partly dissolved, and what passed through the filter, as well as what remained on it, contained muriatic acid. If I heated the blue mass more strongly, it became gray, and was deprived of its solubility in water as well as of its muriatic acid. After several unsuccessful trials varied in different ways, I was at length led to the object I sought by the consideration of a simple fact, namely, that a solution of molybdic acid assumes a blue colour by the contact of most metals. I conceived it would be the same with molybdena itself, and that this metal, participating in the oxigen of the molybdic acid, would change it to the state of blue oxide.

Could not be entirely separated.

Most metals change the molybdic acid blue.

12 grs. of molybdena and 24 of its acid in water form blue oxide.

Exp. 25. In consequence I took twelve grains of metallic molybdena and twenty-four of molybdic acid, reduced the whole to a very fine powder, and put this into seven ounces of water. After standing ten minutes the liquor assumed a blue colour, which grew deeper and deeper. On boiling

boiling for half an hour the solution was found to be much stronger than in any of the preceding trials; and on boiling it a second time the whole of the acid and metal had disappeared, except two or three grains, being converted into blue oxide.

I was now desirous of trying, whether I could not obtain the blue oxide in a still simpler and cheaper manner, by employing the brown oxide obtained from the decomposition of molybdate of ammonia instead of metallic molybdena.

Exp. 26. A hundred grains of molybdic acid and eighty of brown oxide were triturated together, and reduced to a very fine powder. This powder being wetted, after some time a blue colour appeared, but much more tardily than when metallic molybdena was employed. After triturating this mass however for a quarter of an hour, the magma was very blue. It was then boiled four times, with four ounces of water each time, and the whole was dissolved except a few grains. The solutions were blue.

100 grs. of acid and 80 of brown oxide did the same, but more slowly.

Several other trials convinced me, that molybdena in the metallic state acts more powerfully than the brown oxide in converting molybdic acid into blue oxide. I found too, that by long triturating a mixture of metallic molybdena and brown oxide, and adding water from time to time, so as to keep the mixture of the consistence of pap, the greater part of the mass might be converted into blue oxide.

Molybdena converts the brown oxide into blue by trituration in water.

When the mixture was dry, I poured extremely pure water on it, when a smell nearly resembling that of oil of rosemary, faintly inclining to that of camphor, was very sensibly emitted. This is a very extraordinary circumstance; but if any one should doubt the fact, I can appeal to the testimony of Messrs. Trommsdorff and Haberlé, who were with me when I made the experiment. Perhaps the cause of this might be discovered by operating with a larger quantity of materials.

Extraordinary smell from it.

Exp. 27. I took all the solutions of blue oxide in pure water produced in the preceding experiments, poured them into a porcelain capsule, and boiled them down to the consistence of a sirup. As the liquor boiled it grew lighter coloured, till at length it appeared of a deep steel gray;

The blue solutions evaporated.

and after it was cold it resembled altogether a concentrated solution of acetite of copper inclining a little to blue, in other words, it was of a deep blueish green; it was of a metallic and bitter taste; and no precipitate was formed. The addition of a little muriatic acid appeared to restore the original blue colour. This experiment evidently shows, that the blue oxide is capable of passing to a higher degree of oxidation by the effect of simple boiling in water; and that this degree of heat must if possible be avoided, when we wish to obtain blue oxide. Several other experiments, which it would be superfluous to detail here, taught me, that the following process is best adapted to produce a pure and permanent blue oxide.

Best process for obtaining pure and permanent blue oxide.

Take one part of metallic molybdena and two parts of pure molybdic acid (or three parts of brown oxide and four of acid), triturate them a considerable time in a porcelain or glass mortar, moistening the mixture with distilled water, either at the beginning or after it is reduced to a fine powder, so as to give it the consistency of pap. Continue the trituration with a moderate heat, till the matter is very blue. Then add eight or ten parts of water, and boil for a few minutes. After the liquor has stood a little while, filter it, and continue to triturate and lixiviate the residuum, till no more blue solution is obtained. All the solutions being poured into a porcelain capsule, they are to be evaporated at a heat of 30° or 40° of R. [100° or 122° F.], when the colour will undergo no sensible change, and a very fine blue residuum will be obtained, which is soluble in a very small quantity of water. Care must be taken, that the evaporation does not go on too slowly; for I think I have observed, that in consequence of being in contact with the oxigen of the atmosphere, the blue oxide passed gradually to green, yellow, and lastly even to white molybdic acid. At least I have remarked these phenomena, when potash or ammonia was present in the blue solution.

Evaporation must not be too slow.

Means of preventing too high oxidation.

This accident however may be prevented effectually, by leaving a little metallic molybdena or brown oxide in contact with the solution evaporated, till the liquor has attained the consistence of a sirup. This will prevent the oxigen present from producing a higher degree of oxidation.

From

From the experiments on the blue oxide, that have been related, we may deduce the following results.

General results.

1. Several of the degrees of oxidation before observed have been confirmed, and some others discovered. In the experiments made on metallic molybdena I have frequently remarked, that its surface lost its splendour, and seemed to become coated with a *gray* matter.

Different stages of oxidation.

This is certainly a commencement of oxidation, and is the first stage: the *brown* oxide is the next: and this passes, as has been said, by boiling to the *blue;* which may likewise be produced by heating the metal, or by heating the brown oxide obtained by the decomposition of the molybdate of ammonia; and it appears, that the substance produced by these two different operations may be considered as identical. After the blue oxide we have the *blueish green*, which may be produced by boiling the blue, or leaving it exposed some time to the air: and the contact of metallic molybdena, or the action of pure ammonia, will bring this back again to the state of blue oxide. Lastly the blueish green oxide passes to *yellow*, and afterward to *white*, which is the molybdic acid. The transmutation of the blue oxide into the last two is singularly promoted by the presence of an alkali.

Alkali promotes a higher oxidation.

2. The white molybdic acid placed in contact with the brown oxide, or with metallic molybdena, divides its oxigen with them, and thus passes to the state of blue oxide. The blue colour, that molybdic acid acquires on the addition of a metallic solution, as remarked by Scheele, Heyer, and Ilsemann, is an effect of a similar disoxigenation. Other disoxigenizing circumstances may occasion the conversion of the molybdic acid to the state of blue oxide, as for instance the passing of ammoniacal gas over it.

Molybdic acid converted to blue oxide.

After having discovered these different degrees of oxidation, it appears an object to ascertain the proportion of oxigen to the metal in each. This I shall pursue with some other inquiries, when I have procured a sufficient quantity of the ore of molybdena. The principal subjects of my research will be the blue and the brown oxide, as they are the most stable, and are most easy to procure in large quantity and unmixed: but what renders them particularly interesting

Proportions of oxigen a subject of farther inquiry.

teresting is, that they frequently occur in various operations on molybdena, I shall confine myself here to a few of the principal properties of the blue oxide. 1. It comports itself altogether as an acid. It reddens blue paper more quickly and more powerfully than the white acid; and it produces a brisk effervescence on combining with alkaline carbonates, with which it furnishes a blue solution. We see here a base combined with a certain quantity of oxigen manifesting a stronger acidity, than when it contains a greater quantity of the acidifying principle; a very remarkable anomaly. 2. This acidity still remains when the blue oxide has passed to the state of blueish green oxide (which reverts to its former state on the addition of an alkaline corbonate). Its preparation shows its solubility in water, but I have not yet ascertained the quantity water will take up.

Blue oxide acts as an acid more powerfully than the acid itself.

Blueish green oxide also acid.

Exp. 28. The manner in which metallic molybdena comports itself, when heated in the open air, has already been seen. Some phenomena, that occurred when I was ascertaining its specific gravity, led me to examine what would take place on leaving it in contact with water at the ordinary temperature. For this purpose I took thirty grains of powdered molybdena, put it into a porcelain capsule, and wetted it with water, which I left to evaporate slowly. Having poured fresh water upon it, this afterward acquired a blue colour: and this process being several times repeated, the whole of the metal was converted into blue oxide. The different degrees of intermediate oxidation observed in my other experiments did not occur here. The brown oxide, treated in the same manner, produced a similar result.

Molybdena converted into blue oxide by water at the common temperature.

Brown oxide the same.

(To be concluded in our next.)

VI.

Remarks on the Formation of Acetous Acid in Cases of Indigestion; by Mr. PERPERES, *Apothecary, at Azilles. Communicated by Mr.* PARMENTIER*.

CHEMISTS at present are agreed in opinion, that acetic acid is formed during the digestion of certain substances;

Acetic acid supposed to be formed in the stomach

* Annales de Chimie, vol. LX, p. 280.

stances; the experiments here given therefore only confirm what has already been advanced. They do not equally admit however, that the spirituous fermentation may take place there. This our author asserts; but as it is a mere assertion, the subject deserves farther inquiry.

Physiologists have long been convinced, that fermentation is necessary to digestion, and that it may be of the spirituous, acid, or putrid kind. Some alimentary substances are liable to all three, as several observations show: but in general every article of food undergoes that fermentation, which is most analogous to its nature. Vinous, acid, and putrid fermentations take place in it.

As we are acquainted with one kind of ſpirituous fermentation only, and one of putrid, I shall make no mention of these two fermentations. But the same cannot be said of the acid fermentation, since it gives rise to several acids differing in their nature, with which it is of importance to become acquainted, to fix the opinion of chemists on this head. Accordingly I determined to make on myself the experiments I am about to describe. Experiment to ascertain the nature of the acid formed in indigestion.

1. Knowing that my stomach is incapable of digesting roasted chesnuts without great difficulty, occasioning always eructations, followed in a few instants by insupportable heartburn; I took eight ounces, which I ate without bread, after fasting thirteen hours, and having my stomach perfectly empty. An hour and half after eating them I experienced a swelling in my stomach, an effect that amylaceous substances commonly have with me. This indicating the production of some gas, I prepared to collect it in the following manner. I took a funnel with a long semicircular pipe, the small end of which opened under a jar filled with water in the pneumatic trough: and I took care to fill the trough with water, so as to cover the whole of the tube, that I might lose none of the gas, which I knew I should emit. In fact, a few minutes after I felt my stomach still more dilated, and soon had eructations, which I collected by putting my mouth into the funnel. Thus I obtained at different times a cubic inch of gas, which had all the characters of carbonic acid, and a small portion of atmospheric air, which the food always carries down with it, as being necessary to digestion. Eight ounces of roasted chesnuts eaten. Their effects. Carbonic acid gas produced.

The

Heartburn.

The swelling of my stomach being diminished by the abstraction of this carbonic acid gas and atmospheric air; and the acid fermentation appearing from the heartburn I began to feel, to have gone through all its stages; I prepared to follow up my experiment.

The contents of the stomach examined.

2. The object was to ascertain the nature of the acid contained in my stomach: and this could only be done by bringing it up, that I might find out its specific characters by subsequent examination.

Accordingly I took twenty grains of ipecacuanha, mixed with three ounces of distilled water, at one dose: and a quarter of an hour after I drank warm distilled water to the quantity of fourteen ounces, without producing any effect. Three ounces more however made me bring up at twice all I had taken.

Part perhaps had been absorbed.

On weighing the whole, I found it amounted to two ounces less than I had eaten and drank. Whether the stomach had digested the two ounces of fluid that were deficient, or they had been absorbed, I cannot say.

Appearance.

What I had brought up resembled feculæ diffused in water; which showed, that the fermentation had destroyed the alimentary substance I had eaten, particularly as the smell was strongly acetous. This smell confirmed the idea I had long conceived, that vinegar is formed in cases of indigestion, and encouraged me to go through with my experiments.

An acid present.

3. I dipped litmus paper into it, which was immediately reddened. A little dropped into an infusion of violets reddened it instantly. Thus I was convinced of the existence of an acid, and I had recourse to the following means for ascertaining its nature.

Distilled.

4. Having put the whole into a glass retort, I adapted to it a globular receiver, furnished with a tube of safety, and with a curved tube terminating under a jar in the pneumatic apparatus, to receive the gas that might be dissolved in the matter I was examining. The whole being luted, I heated it gradually till it boiled; and kept it in this state till the matter in the retort began to acquire consistency. I then unluted my apparatus, and found in the receiver sixteen ounces and half of a very clear fluid, the smell

Products vinegar,

smell and taste of which resembled those of distilled vinegar, and which had all the properties of an acid. The only aeriform product was a little carbonic acid, which was very distinguishable by the rapidity with which the bubbles rose through the water, when it was once saturated, as well as by their magnitude. and carbonic acid.

5. Though the smell and taste of the product of the distillation already gave me strong proofs of the existence of acetous acid, I thought it necessary, to have farther confirmation. I therefore took some soda purified by means of alcohol, and added it to the whole of the liquor to supersaturation. I then filtered, and evaporated to a proper degree for obtaining crystals of acetate of soda. I put the porcelain vessel, that contained the saline liquor, in a cool place; and the next day, to my great satisfaction, I found, that the form of the crystals, which were striated prisms much resembling very small crystals of sulphate of soda, was precisely that of acetate of soda. Their taste too was bitter, pungent, and giving an acrid taste at the beginning, which afterward finished with being alkaline; in short they were in every respect similar to acetate of soda prepared directly from its component parts. The acid saturated with soda.

6. Still fearing, that these experiments might not be sufficiently demonstrative, I was desirous of satisfying myself still farther. I therefore took half an ounce of the saline substance I had obtained, and dissolved it in six ounces of distilled water. This solution I divided into two equal portions: into one I poured gradually very fine sulphuric acid, but not sufficient to decompose it entirely; and into the other my solutions of barytes. The first portion, which had been decomposed by sulphuric acid from its greater affinity with soda, was put into a small retort, to which I adopted a receiver, and distilled with a gentle heat. The product in the receiver was acetous acid, perfectly pure, and with a very fragrant smell, in fact having all the characters of that acid. Into the second I poured a solution of barytes, till the soda was set free: and into the phial containing the solution of soda and acetate of barytes I poured a sufficient quantity of alcohol to dissolve the soda, and precipitate the acetate of barytes. Thus I was completely The salt dissolved, divided into two portions, one decomposed by sulphuric acid and distilled, the other by barytes.

pletely satisfied, that it was acetous acid; and all my suspicions were realized.

Conclusions. From the experiments I have related it follows:

The distension of the stomach owing to carbonic acid gas: 1. That the distension experienced by the stomach in cases of indigestion is occasioned by the formation of carbonic acid, arising from a commencement of decomposition, which the nutritive substances taken as food, chiefly when they are of the amylaceous vegetable kind, have undergone.

and the heart-burn to acetous acid. 2. That the burning pain which the digestive organ experiences, and which sometimes extends to the œsophagus, is owing to a quantity of acetous acid, that is formed by the complete disoxigenization of the aliment.

8 oz. of chesnuts produced 2 $\frac{3}{4}$ oz. of acetous acid. 3. That eight ounces of roasted chesnuts produced two ounces and six drachms of acetous acid, after having fermented in the stomach an hour and half.

Remedy. 4. Finally, that the method of remedying this disagreeable sensation, which frequently occurs to persons who have weak stomachs, is to take after a meal ten grains of powdered colombo-root, with twelve grains of calcined magnesia, mixed together for a single dose. This remedy has constantly succeeded with me.

VII.

On the Cause of Animal Heat. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Query respecting animal heat. DR. Reade in his observations in your last Number, on the generation of heat in water by agitation, offers a query; "Whether friction be adequate to account for animal heat."

Friction not adequate to its production. If the Dr. mean by friction, that which is produced by the impetus or flow of blood in the arteries, it would appear very improbable, that the friction thus produced should be capable of supporting animal heat: on the contrary, I have been induced to believe, from analogous experiments, that animal heat is produced and supported by *gradual combustion*, produced by the action of the oxigen of the atmospheric air, which is inhaled by the lungs, on the iron of the blood,

Owing to combustion of iron in the blood.

blood. At each inspiration a new action is produced: and the attraction of oxigen for iron needs no observation from

Your very obedient humble servant,

Bristol, June 16th, 1806. P.

ANNOTATION.

THE supposition, that animal heat is kept up by a process analogous to combustion is by no means new: but it is far from probable, that it should be by the combustion of iron. If we consider the quantity of heat necessary to preserve the warmth of so large a mass as the human body under certain circumstances, and requiring constant renovation, where shall we find a sufficient supply of iron, to generate by its combustion the heat actually produced in many cases? or what becomes of this iron afterward? Though iron appears to be pretty generally diffused, and to enter in small portions into perhaps most animal and vegetable substances: yet the ingesta, particularly in fevers, where much heat is produced, and little nourishment taken, can scarcely afford enough to account for the heat, or for the oxigen consumed in the air vitiated by respiration. And on the other hand the proportion of oxide of iron in the blood will scarcely be found answerable to the supposed effect. Facts however are always valuable; and a series of well conducted experiments, whatever they may tend to prove, or whatever hypotheses they may contradict, cannot fail to be interesting to the impartial inquirer. This not probable.

VIII.

Description of a Species of Ox, named Gayál. Communicated by H. T. Colebrook, *Esq**.

THE *gayál* was mentioned in an early volume of the researches of the *Asiatic* Society†, by its *Indian* name, which Gayál has been mentioned but not described.

* Abridged from the Asiatic Researches, vol. VIII.

† In the second volume, (p. 188,) published in 1790.

was

was explained by the phrase, 'cattle of the mountains.' It had been obscurely noticed (if indeed the same species of ox be meant,) by Knox, in his historical relation of *Ceylon**; and it has been imperfectly described by Captain Turner, in his journey through *Bootan*†. Herds of this species of cattle have been long possessed by many gentlemen, in the eastern districts of *Bengal*, and also in other parts of this province: but no detailed account of the animal, and of its habits, has been yet published in *India*. To remedy this deficiency, Dr. Roxburgh undertook, at my solicitation, to describe the *gayál*, from those seen by him in a herd belonging to the Governor General. Dr. Buchanan has also obligingly communicated his observations on the same cattle: and both descriptions are here laid before the society; with information obtained from several gentlemen at *Tipura*, *Silhet*, and *Chatgaon*, relative to the habits of the animal.

A distinct species.

From the information which was first received, it was supposed that the *gayál* would not engender either with the buffalo, or with the common bull and cow, and must therefore constitute a distinct species in every system of classification. Although this is contradicted by the correcter information now obtained, yet on account of the considerable, and apparently permanent, difference between the common cow and the *gayál*, this ought still, perhaps, to be considered as a distinct species, rather than as a variety.

Description by Dr. Roxburgh.

'The *gayál*,' says Dr. Roxburgh, 'is nearly of the size and shape of the *English* bull. It has short horns, which are distant at their bases, and rise in a gentle curve directly out and up: a transverse section, near the base, is ovate; the thick end of the section being on the inside. The front is broad, and crowned with a tuft of lighter coloured, long, curved hair. The dewlap is deep and pendent. It has no mane, nor hump; but a considerable elevation over the withers. The tail is short; the body covered with a tolerable coat of straight, dark-brown hair: on the belly, it is lighter coloured; and the legs and face are sometimes white.'

Doctor Buchanan thus describes it:

* P. 21. † Embassy to *Tibet*, p. 160.

The

'The *gayál* generally carries its head with the mouth projecting forward like that of a buffalo. The head, at the upper part, is very broad and flat, and is contracted suddenly towards the nose, which is naked, like that of the common cow. From the upper angles of the forehead proceed two thick, short, horizontal processes of bone, which are covered with hair. On these are placed the horns, which are smooth, shorter than the head, and lie nearly in the plane of the forehead. They diverge outward, and turn up with a gentle curve. At the base they are very thick, and are slightly compressed, the flat sides being toward the front and the tail. The edge next the ear is rather the thinnest, so that a transverse section would be somewhat ovate. Toward their tips, the horns are rounded, and end in a sharp point. The eyes resemble those of the common ox; the ears are much longer, broader, and blunter than those of that animal. By Dr. Buchanan. Horns.

'The neck is very slender near the head, at some distance from which a dewlap commences; but this is not so deep, nor so much undulated, as in the *bos zebu*, or *Indian* ox. The dewlap is covered with strong longish hair, so as to form a kind of mane on the lower part of the neck; but this is not very conspicuous, especially when the animal is young. Neck.

'In place of the hump, which is situated between the shoulders of the *zebu*, the *gayál* has a sharp ridge, which commences on the hinder part of the neck, slopes gradually up till it comes over the shoulder joint, then runs horizontally almost a third part of the length of the back, where it terminates with a very sudden slope. The height of this ridge makes the neck appear much depressed, and also adds greatly to the clumsiness of the chest, which, although narrow, is very deep. The sternum is covered by a continuation of the dewlap. The belly is protuberant, but in its hinder part is greatly contracted. The rump, or *os sacrum*, has a more considerable declivity than that of the *European* ox, but less than that of the *zebu*. Body.

'The tail is covered with short hair, except near the end, where it has a tuft like that of the common ox; but in the *gayál*, the tail descends no lower than the extremity of the *tibia*. Tail.

'The legs, especially the fore ones, are thick and clumsy. Legs.
The

The false hoofs are much larger than those of the *zebu.* The hinder parts are weaker in proportion than the forehand; and, owing to the contraction of the belly, the hinder legs, although in fact the shortest, appear to be the longest.

Hair. 'The whole body is covered with a thick coat of short hair, which is lengthened out into a mane on the dewlap, and into a pencil-like tuft on the end of the tail. From the summit of the head there diverges, with a whirl, a bunch of rather long coarse hair, which lies flat, is usually lighter coloured than that which is adjacent, and extends towards the horns, and over the forehead.

Colour. The general colour of the animal is brown, in various shades, which very often approaches to black, but sometimes is rather light. Some parts, especially about the legs and belly, are usually white, but in different individuals, they are very differently disposed.

Size. 'In the first column of the following table is the measurement of a full grown cow: in the second is that of a young male.

	Ft.	In.	Ft.	In.
From the nose to the summit of the head	1	6	1	8
Distance between the roots of the horns····	0	10	0	9
From the horns to the shoulder············	3	3	3	0
From the shoulder to the insertion of the tail	4	3	3	10
Height at the shoulder···················	4	9	4	7
Height at the loins·····················	4	4	4	2
Depth of the chest······················	2	9	-	-
Circumference of the chest···············	6	7	5	7
Circumference at the loins···············	5	10	5	6
Length of the horns·····················	1	2	-	-
Length of the ears······················	0	10	-	-

Distinguishing characters of the species of ox. 'The different species of the ox kind may be readily distinguished from the *gayál* by the following marks. The *European* and *Indian* oxen by the length of their tails, which reach to the false hoofs; the *American* ox by the gibbosity on its back; the *boves moschatus, Cafer,* and *pumilus,* by having their horns approximated at the bases; the *bos grunniens* by its whole tail being covered with long silky hairs; the *bos bubalus,* at least the *Indian* buffalo, by having the whole length of its horns compressed, and by their being longer

longer than the head, and wrinkled; also by its thin coat of hair, by its want of a dewlap, and, above all, by its manners; the *bos barbatus* by the long beard on its chin.

'The cry of the *gayál* has no resemblance to the grunt of the Indian ox, but a good deal resembles that of the buffalo. It is a kind of lowing, but shriller, and not near so loud as that of the European ox. To this, however, the *gayál* approaches much nearer than it does to the buffalo.' Voice.

The result of inquiries made by Mr. Macrae, at *Chatgaon*, has been communicated by that gentleman, in the following answer to questions which were transmitted to him.

'The *gayál* is found wild in the range of mountains that form the eastern boundary of the provinces of *Aracan*, *Chittagong (Chatgaon)*, *Tipura*, and *Silhet*. Native place.

'The *Cúcis*, *or Lunctas*, a race of people inhabiting the hills immediately to the eastward of *Chatgaon*, have herds of the *gayál* in a domesticated state. By them he is called *shïal*; from which, most probably, his name of *gayál* is derived; as he is never seen on the plains, except when brought there. By the *Mugs* he is named *j'hongnuah;* and by the *Burmas*, *núnee*. In the *Hindu s'ástra* he is called *gabay*. It appears, however, that he is an animal very little known beyond the limits of his native mountains, except to the inhabitants of the provinces abovementioned. Domesticated.

'The *gayál* is of a dull heavy appearance; but, at the same time, of a form which indicates much strength and activity, like that of the wild buffalo. His colour is invariably brown, but of different shades, from a light to a dark tinge; and he frequently has a white forehead, and four white legs, with the tip of the tail also white. He has a full eye, and, as he advances in age, often becomes blind; but it is uncertain whether from disease, or from a natural decay. His disposition is gentle; even when wild, in his native hills, he is not considered to be a dangerous animal, never standing the approach of man, much less bearing his attack. The *Cúcis* hunt the wild ones for the sake of their flesh. Appearance. Habits.

'The *gayál* delights to range about in the thickest forests, where he browses, evening and morning, on the tender shoots

shoots and leaves of different shrubs; seldom feeding on grass, when he can get these. To avoid the noonday heat, he retires to the deepest shade of the forest; preferring the dry acclivity of the hill to repose on, rather than the low swampy ground below; and never, like the buffalo, wallowing in mud.

Milk. 'The *gayál* cow gives very little milk, and does not yield it long; but what she gives is of a remarkable rich quality, almost equally so with the cream of other milk, and which it also resembles in colour. The *Cúcis* make no use whatever of the milk, but rear the *gayáls* entirely for the sake of

Hide. their flesh and skins. They make their shields of the hides

Flesh. of this animal. The flesh of the *gayál* is in the highest estimation among the *Cucis*; so much so, that no solemn festival is ever celebrated without slaughtering one or more *gayáls*, according to the importance of the occasion.

Turned loose. 'The domesticated *gayáls* are allowed by the *Cúcis* to roam at large during the day through the forest in the neighbourhood of the village; but, as evening approaches, they all return home of their own accord; the young *gayál* being early taught this habit, by being regularly fed every

Fond of salt. night with salt, of which he is very fond: and, from the occasional continuance of this practice, as he grows up, the attachment of the *gayál* to his native village becomes so strong, that, when the *Cúcis* migrate from it, they are obliged to set fire to the huts which they are about to leave, lest their *gayáls* should return thither from their new place of residence, before they become equally attached to it, as to the former, through the same means.

Food. 'The wild *gayál* sometimes steals out from the forests in the night, and feeds in the rice fields bordering on the hills. The *Cúcis* give no grain to their cattle. With us, the tame *gayáls* feed on *caláï (phaseolus max.)*; but, as our hills abound with shrubs, it has not been remarked what particular kind of grass they prefer.

Another species wild. 'The *Hindus*, in this province, will not kill the *gabay*, which they hold in equal veneration with the cow. But the *as'l gayál*, or *seloï*, they hunt and kill, as they do the wild buffalo. The animal here alluded to is another species of *gayál* found wild in the hills of *Chatgaon*, a correct description

description of which will be given hereafter. He has never been domesticated; and is, in appearance and disposition, very different from the common *gayál*, which has been just described. The natives call him the *asl gayál* in contradistinction to the *gabay*. The *Cúcis* distinguish him by the name of *seloi*, and the *Mugs* and *Barmas* by that of *p'hanj*: and they consider him, next to the tiger, the most dangerous and the fiercest animal of their forests.' And very fierce.

Mr. Elliott says: 'I have some *gayáls* at *Munnamutty*, and, from their mode of feeding, I presume, that they keep on the skirts of the vallies, to enable them to feed on the sides of the mountain, where they can browse. They will not touch grass, if they can find shrubs. Browse.

'While kept at *Camerlah*, which is situate in a level country, they used to resort to the tanks, and eat on the sides, frequently betaking themselves to the water, to avoid the heat of the sun. However, they became sickly and emaciated, and their eyes suffered much; but, on being sent to the hills, they soon recovered, and are now in a healthy condition. They seem fond of the shade, and are observed in the hot weather to take the turn of the hills, so as to be always sheltered from the sun. They do not wallow in mud like buffaloes; but delight in water, and stand in it, during the greatest heat of the day, with the fronts of their heads above the surface. Require a hilly country.

I take this opportunity, while treating of a species of ox, to notice an errour which crept into Kerr's unfinished translation of the animal kingdom in Linnæus's Systema Naturæ; and which has been followed by Doctor Turton in translating the general system of nature by Linnæus. Mr. Kerr described and figured, under the name of *bos arnee*, an animal, which, notwithstanding the exaggerated description, given on the authority of 'a British officer, who met with one in the woods, in the country above *Bengal**,' is evidently nothing else but the wild buffalo, an animal very common throughout *Bengal*, and known there, and in the neighbouring provinces of *Hindostan*, by the name of *arna*. Though neither fourteen feet high, as Mr. Kerr has Mistake of Kerr and Turton.

* Kerr, page 336.

stated,

stated, or rather as the officer, on whose information he relied, had affirmed; nor even eight feet high, as Doctor Turton, following Kerr's inference from a drawing, asserts; yet it is a large and very formidable animal, conspicuous for its strength, courage, and ferocity. It may not be true, that the buffaloes of *Asia* and *Europe* constitute a single species; but, certainly, the wild and tame buffaloes of *India* do not appear to differ in any thing, except the superior size and more uniform figure of the wild animal. A better description of the buffalo, than has yet been given, is perhaps wanted; but the *bos arnee* of Kerr and Turton must be rejected from systems of *zoology*, as an erroneous description taken from a loose drawing, assisted by the fragment of a skeleton.

Good description of the buffalo wanted.

IX.

A concise Method of determining the Figure of a gravitating Body revolving round another. By a Correspondent.

To Mr. NICHOLSON.

SIR,

Deficiency of Newton's researches.

IT is well known, that there are some imperfections in Sir Isaac Newton's investigations respecting the figures of gravitating bodies, which have been supplied by Maclaurin and Clairaut: the subject is however still considered as difficult and intricate, and the simplest calculations respecting it have hitherto been too prolix, to be distinctly conceived as links of the same chain. I shall endeavour to point out a method of treating it which is extremely compendious and convenient.

Forces concerned.

Neglecting in the first place the diurnal rotation, we may suppose, that each particle of the body revolves in an equal orbit, so that its centrifugal force may be equal to the mean attractive force; then the local attractive force will be greater or less than this by a difference which must obviously be proportional to the distance from an equatorial plane perpendicular to the direction of the central body, and tending

to

to remove the body from this plane. A second disturbing force will also arise from the want of parallelism in the direction of the attractive force, which will tend towards the line joining the centres, and will be every where to the whole force as the distance from this line to the distance of the bodies. Now if each of these forces be reduced to the direction of the circumference of the sphere, from which the figure is supposed to vary but very little, it will be every where proportional to the product of the sine and cosine of the distance from the equatorial plane, and when this distance is half a right angle, each of them will be half as great as in its intire state. Thus the gravitation towards the moon at the earth's surface is to the gravitation towards the earth as 1 to 70 times the square of $60\frac{1}{2}$, or to 256217, and the first disturbing force is to the whole of this as 2 to $60\frac{1}{2}$, at the point nearest to the moon, and the second as 1 to $60\frac{1}{2}$ at the equatorial plane; and the sum of both reduced to the direction of the circumference where greatest, as 3 to 121, that is, to the whole force of the earth's gravitation as 1 to 10,334,000. And in a similar manner the joint disturbing force of the sun is to the weight as 1 to 25,736,000.

Now if a sphere be inscribed in an oblong spheroid, the elevation of the spheroid above the sphere must obviously be proportional, if measured in a direction parallel to the axis of the spheroid, to the ordinate of the sphere, that is, to the sine of the distance from its equator; and if reduced to a direction perpendicular to the surface of the sphere, it must be proportional to the square of that sine; and the tangent of the inclination to the surface of the sphere, which is as the fluxion of the elevation divided by that of the circumference, must be expressed by twice the continual product of the sine, the cosine, and the ellipticity or greatest elevation, the radius being considered as unity: so that the ellipticity will also express the tangent of the inclination where it is greatest; and the inclination will be every where as the product of the sine and cosine.

Inclination of the surface of a spheroid.

If therefore the density of the elevated parts be considered as evanescent and their attraction be neglected, there will be an equilibrium when the ellipticity is to the radius as the disturbing force to the whole force of gravitation: for each

Tides of a sea of inconsiderable density.

particle situated on the surface will be actuated by a force preceisely equal and contrary to that which urges it in the direction of the inclined surface. Hence, if the density of the sea be supposed inconsiderable in comparifon with that of the earth, the radius being 20,839,000 feet, the height of a solar tide in equilibrium will be 2·0166 feet, and that of a lunar tide ·8097.

Attraction of the elevated parts.

We must next inquire what will be the effect of the gravitation of the elevated parts, on any given supposition respecting their density. Let us imagine the surface to be divided by an infinite number of parallel and equidistant circles, beginning from any point at which a gravitating particle is situated, and let their circles be divided by a plane bisecting the equatorial plane of the spheroid; it is obvious that if the elevations on the opposite sides of this plane be equal in each circle, no lateral force will be produced; but when they are unequal, the excess of the matter on one side above the matter on the other will produce a disturbing force. The elevation being every where as the square of the distance from the equatorial plane, the difference, corresponding to any point of that semicircle in which the elevation is the greater, will be as the difference of the squares of the distances of the corresponding points of the two semicircles, that is, as the product of the sum and the difference of the distances: but the sum is twice the distance of the centre of the circle from the equatorial plane, or twice the sine of the distance of the gravitating particle from the plane, reduced in the ratio of the radius to the cosine of the angular distance of the circle from its pole; and the difference is twice the actual sine of any arc of the circle, reduced to a direction perpendicular to that of the plane, that is, reduced in the proportion of the radius to the cosine of the angular distance of the given particle from the equatorial plane. From these proportions it follows, that, in different positions of the gravitating particle, the effective elevation at each point of the surface, similarly situated with respect to it, is as the product of the sine and cosine of its angular distance from the equatorial plane, the other quantities concerned remaining the same in all positions: the disturbing attraction of all the prominent parts varies therefore precisely

cisely in this ratio, the matter which produces it being always similarly arranged, and varying only in quantity; consequently the sum of this attraction and the original disturbing force both vary as the inclination of the surface, and may be in equilibrium with the tendency to descend towards the centre, provided that the ellipticity be duly commensurate to the density of the elevated parts.

Magnitude of the ellipticity.

In the last place we must investigate what is the magnitude of the ellipticity corresponding to a given disturbing force and a given density. It follows from the proportions already mentioned, first, that the effectual elevation at each point of each concentric semicircle is proportional to the sine of its distance from the bisecting plane; and secondly, that the greatest effective elevation of each semicircle, for any one position of the superficial particle, is as the product of the sine and the cosine of the angular distance from that particle, the diameter of the circle being as the sine, and the distance of its centre from the equatorial plane as the cosine. It may easily be shown, that the disturbing force, reduced to the direction of the surface, or of the plane of each circle, is equal to the attraction which would be exerted by the matter covering the whole semicircle to a height equal to half the greatest elevation, if placed at the middle point: for the elevation being as the sine of the distance from the bisecting plane, and the comparative effect being also as the sine, the attraction for each equal particle of the semicircle is as the square of the sine, and the whole sum half as great as if each particle produced an equal effect with that on which the elevation is greatest. We must therefore compute the attraction of the quantity of matter thus determined, supposing it to be disposed at the respective points of a great circle passing through the given point and the pole of the spheroid. The immediate attraction of each particle being inversely as the square of the chord, its effect reduced to the common direction will be as the sine directly, and the cube of the chord inversely, and this ratio being compounded with that of the product of the cosine and the square of the sine, which expresses the quantity of matter at each point, the comparative effect will be as the cube of the sine and the cosine directly, and as the cube of the

 chord

chord inversely, or as the cube of the cosine of half the arc and the cosine of the whole arc conjointly. If therefore we call the cosine of half the arc x, the cosine of the whole arc will be $2x^2-1$, and the fluxion of the arc being $-\frac{2\dot{x}}{\surd(1-xx)}$, that of the force will be $\frac{2x^3\dot{x}-4x^5\dot{x}}{\surd(1-xx)}$, of which the fluent is $(\frac{4}{5}x^4+\frac{2}{5}x^2+\frac{4}{5})\surd(1-xx)$, as may be shown by substituting, in the reduction of its fluxion, $\frac{1-xx}{\surd(1-xx)}$ for $\surd(1-xx)$: and while x decreases from 1 to 0, this fluent becomes $\frac{4}{5}$. But in order to determine the unit with which this quantity is to be compared, we must consider the initial force as unity, and imagine, that it is continued through an arc equal in length to the radius; and we must find the attraction of the solid contained between a circular plane and a conical surface, initially touching the effective portion of the elevation, and including it between them; the attraction reduced to a common direction, being initially half the whole attractive force of such a solid, as we have already seen of the concentric circles considered separately. But the attraction of any slender conical or pyramidical body for a particle placed at its vertex, is three times as great as that of the same quantity of matter situated at its base; consequently the attraction of the supposed solid is equal to that of the circumscribing semicylinder placed at the distance of the radius: the conical excavation being half of the solid, and the semicylinder triple of the cone: but the height of this semicylinder in the case of a particle situated half way between the pole and the equator of the spheroid, is twice the ellipticity, the tangent of the angle of mutual inclination of the surfaces of the effective elevation being initially equal to twice the greatest ordinate, because the product of the sine and cosine, when greatest, is equal to half of the radius: the semicylinder will therefore be equal to a cylinder of which the diameter is equal to that of the sphere, and the height equal to the ellipticity; and the contents of this cylinder will be to that of the sphere, as $\frac{3}{4}$ of the ellipticity to the radius. Such therefore is the unit with which the disturbing attraction is to be compared; and when the densities

sities are equal, this force will be to the whole weight as $\frac{3}{4}\cdot\frac{4}{5}$ or $\frac{3}{5}$ of the ellipticity to the radius; and the portion of the inclination remaining to be compensated by the primitive disturbing force will be $\frac{2}{5}$ of the whole, so that the ellipticity must be to the proportional disturbing force as 5 to 2. And if the density of the sea be to the mean density of the earth as 1 to n, the disturbing force, produced by its attraction, will be to the ellipticity as $\frac{3}{5n}$ to 1, and the primitive disturbing force as $1-\frac{3}{5n}$ to 1.

The heights of the solar and lunar tides in equilibrium having been found equal to ·8097 and 2·0166 feet respectively, on the supposition of the density of the sea being inconsiderable, they must be increased to 2·024 and 5·042 for an imaginary planet of uniform density; but since n is in reality about $5\frac{1}{2}$, and $\frac{3}{5n}$ nearly $\frac{1}{9}$, the ellipticity must be to the primitive disturbing force only as 1 to $\frac{8}{9}$ or 9 to 8, and the height of the sides in equilibrium ·911 and 2·269 respectively, and the joint height 3·18 feet. And when the surface assumes any other form than that which affords the equilibrium, the force tending to restore that form is always less by one ninth than it appears to be when the attraction of the elevated parts is neglected. The theory of the tides must therefore be very materially modified by these considerations, although they do not affect the general method of explaining the phenomena.

Tides of a homogeneous spheroid, and of the sea as it actually exists.

These calculations are also immediately applicable to the figure of an oblate spheroid: for it may easily be shown, that the difference of the elevations in the opposite halves of each semicircle is precisely the same in an oblate as in an oblong spheroid of equal ellipticity: so that the ellipticity must here also be to the disturbing force, where it is greatest, as 1 to $1-\frac{3}{5n}$, or to the centrifugal force at the equator as 1 to $2-\frac{6}{5n}$. Thus, the centrifugal force being $\frac{1}{289}$, if the density were uniform, the ellipticity would be $\frac{1}{231}$; but since it is in reality about $\frac{1}{311}$, $2-\frac{6}{5n}=\frac{231}{311}$, and $n=1{\cdot}32$,

Ellipticity from centrifugal force, and density of the superficial parts of the earth.

$n = 1{\cdot}32$, n implying here the mean density of the earth compared with the mean density of the elevated portion of the spheroid, which hence appears to be about three fourths of that of the whole earth. It is obvious that, in this case as well as in the former, if the density of the sea were two thirds greater than that of the earth, the slightest disturbing force would completely destroy the equilibrium, and the whole ocean would be collected on one side of the earth.

I am, Sir,

Your very humble servant,

A. B. C. D.

X.

Description of a new Compensation Pendulum; by Lieutenant HENRY KATER. *Communicated by the Author.*

Many attempts to correct the irregularity of clocks from heat and cold.

SINCE the first application of the pendulum to clocks, numerous attempts have been made, to correct the errour arising from a variation of temperature, which, by contracting or dilating the substance of which the pendulum rod is composed, occasions the clock to go faster in cold than in warm weather, and consequently to vary considerably in its rate at different seasons of the year.

Defects of the gridiron pendulum,

The gridiron pendulum, now used in almost all regular observatories, though generally supposed to be the best calculated to remedy this inconvenience, is complex, and requires the greatest nicety in proportioning the brass and steel rods (of which it is composed) to each other; it is very expensive; and, if it should be badly constructed at first, is incapable of adjustment.

and of the mercurial.

The mercurial pendulum is not so liable to the last of these objections, for, if the quantity of mercury should not be found exactly to compensate for the expansion of the metallic rod, a little may be added or taken away; but then it must require very numerous trials, before an accurate result can be obtained.

A simple,

These considerations induced me, to turn my attention to the

the subject, and to endeavour to construct a pendulum, which should unite simplicity and cheapness, with the capability of being easily and accurately adjusted.

cheap, and easily, adjusted pendulum desirable.

Wood has been long known as a substance that expands less than any other with heat; and from this property many pendulum rods of time-pieces have been made of wood, and found to answer remarkably well; but it is surprising no advantage has hitherto been taken of this knowledge, to apply a compensation, which might counteract the small expansion to which a pendulum of wood is liable.

Wood advantageous, but no attempt yet to compensate its expansion.

Wood therefore, if it can be rendered perfectly impervious to moisture, appears to be by far the best material, that can be used for the *rod* of the pendulum; and as zinc is a metal which suffers the greatest expansion from an increase of temperature, I consider it preferable to every other, that could be employed as a compensation.

Wood best for the rod, with zinc as a compensation.

The first step was to ascertain accurately the quantity of the expansion of wood, as I could find no experiments on the subject at all satisfactory. For this particular purpose a pyrometer was used, which it would be unnecessary here to describe, calculated to receive a rod of wood four feet in length, one end of which was made to act against the shorter arm of a lever, causing the longer to describe an arc, the divisions of which might easily be read off to the thousandth part of an inch.

Expansibility of wood examined.

A rod of very dry and well seasoned white deal was procured, free from knots, four feet in length, three quarters of an inch in breadth, and a quarter of an inch thick. Each end was exactly squared, and covered with a thin flat plate of brass. This rod was exposed in an oven to the temperature of 235°, and on measuring it in the pyrometer, it was found to have contracted; it was therefore replaced in the oven, and suffered to remain a long time till it appeared a little discoloured, in order to dissipate all moisture. The temperature of the oven was then examined, and found to be still 235°. The deal rod was now quickly removed, and placed in the pyrometer, where it remained a sufficient time to acquire the temperature of the room, which was 49°, when the space described in the interval by the long arm of the lever, was registered; and in this manner by two experiments,

Rod of white deal $\frac{3}{4}$ inch broad and $\frac{1}{4}$ thick

expanded ·0049 inch per foot with 180° of heat.

ments, which gave precisely the same result, the expansion of 4 feet of white deal was determined to be 0·0205 parts of an inch with 186° of Fahrenheit's thermometer, from which by proportion we get 0·0049 parts of an inch for the expansion of one foot with 180° difference of temperature.

I shall now attempt to give a general description of the pendulum, and then proceed to a more particular account of the manner in which it is constructed.

Pendulum described.

A B C D, Pl. V, fig. 1, is cast in zinc. From A to B is one inch; and from C to D nearly two inches: The height of A C is ten inches. Above A B a piece of brass is soldered, an inch square and half an inch thick, through which a hole is made four tenths of an inch in diameter, and tapped with a very fine screw. A cylinder of zinc, E F, about two inches and a half in length, has a screw on it to fit that in the piece of brass as accurately as possible. This cylinder should be carefully turned in a lathe, on a hole as a centre about the eighth of an inch diameter, and made quite through it; the top of the cylinder to the length of a quarter of an inch is filed square, for the purpose of more readily turning the screw with a key, or pincers: and there is a thin plate of brass, represented at *c*, *d*, which screws on the cylinder, in order to fix it firmly at any height.

In the bottom plate of zinc, C D, a hole is made half an inch in diameter, through which the pendulum spring passes, and the whole is fastened by four screws to the cock of the time-piece, which is represented by fig 2, and which is cast for the purpose with the addition of a plate of brass on which the compensation rests.

A steel wire, *g*, *h*, with a fine screw on it, passes through the hole in the cylinder of zinc, by means of which, and the nut below *g*, the pendulum is shortened or lengthened. The watch spring, which supports the pendulum, is fastened to the steel wire at *h*, by means of a pin, and, passing through a slit in the plate of brass on which the compensation rests, is attached to the end of the pendulum rod in the manner hereafter described.

The *rod* of the pendulum is made of white deal, three quarters of an inch broad, and four tenths of an inch thick, and is chosen perfectly free from knots, and well seasoned.

Previous

Previous to its being reduced to the exact dimensions, it is to be baked in an oven, till the surface appears a little charred; and as it is of the utmost importance, that it should be rendered perfectly impervious to moisture, the ends are soaked in melted sealing wax, and the rod, being cleaned, is coated several times with copal varnish. Pendulum described.

The top of the rod is to be divided with a very fine saw, to admit the spring of the pendulum, where it is secured by two or three small pins passing through it and the spring, and rivetted on each side.

The *weight* of the pendulum is of the usual form, and pierced to receive the rod, which is immovably fixed to the centre by means of a screw, passing through it and the weight.

The length of the pendulum is regulated by the screw and nut at the top; but there is also a screw with a less weight at the lower extremity of the pendulum, in order to adjust it with greater accuracy in the usual manner.

Now it is evident, that, if the part which is made of zinc be so proportioned to the other materials of which the pendulum is composed, as to undergo an equal expansion with any increase of temperature, the pendulum will always maintain the same length, and its oscillations, as far as temperature is concerned, will be performed in equal times.

In order to discover the length of zinc necessary to effect this, let the steel screw, g, h, be 9·5 inches long; and the spring 3 inches, making together 12·5 inches. Then, as the expansion of zinc to steel is as 353 to 147, we shall have $\frac{147 \times 12{\cdot}5}{353} = 5{\cdot}2$ inches nearly, for the length of zinc required to correct the expansion of the *steel* employed in the pendulum. Next, as the spring will extend about two inches below the plate of the cock of the time-piece, whence the length of the pendulum is measured, there will remain about 37 inches of deal, and the expansion of deal being to that of zinc as 49 to 353, we have $\frac{49 \times 37}{353} = 5{\cdot}1$ inches nearly; which, being added to 5·2 before found, gives 10·3 inches, the length of zinc which will counteract the expansion of the whole pendulum. Calculation of the length of zinc necessary for the compensation.

The

The length of the zinc may be altered.

The length of the zinc may be varied by means of the screw EF, and experience alone can determine, whether the compensation be accurate, for, if the clock be found to *gain* in warm weather, it is evident, that the zinc is too long, and to correct it the screw E F, must be advanced; and vice versa.

Principles of its adjustment.

The quantity of the alteration requisite may be very nearly determined by knowing how much the clock has varied from its regular rate during a certain period, the difference of temperature, and the measure of one revolution of the screw E F, which last should be previously ascertained with the utmost accuracy. It is scarcely necessary to add, that the pendulum must be as much raised by means of the nut below *g*, as it was lowered by shortening the compensation.

Another mode of applying zinc as a compensation to a wooden rod.

There is another mode of applying a compensation of zinc to a rod of wood, far more simple than that already described; but it appears liable to some objections, which experience may perhaps prove to be unfounded. The rod is made of deal prepared in the manner before mentioned, but is suspended from the cock of the time-piece in the common way by a spring one inch in length.

This compensation described.

A square tube of zinc, represented at fig. 3, is cast seven inches long, and three quarters of an inch square; the *internal* dimensions of the tube are four tenths of an inch each side. The lower part of the pendulum rod remains of the same *thickness*, but is cut away on the two sides for the length of seven inches, so as to slide with perfect freedom in the tube of zinc. A piece of brass, rather more than a quarter of an inch thick, is soldered to the bottom of the tube of zinc at C; and a hole with a fine screw is made through it similar to that before described in fig. 1. A cylinder of zinc of the same description as E F, fig. 1, but only an inch and a half in length, is made to screw into the piece of brass just mentioned, and a thin plate of brass screws on the cylinder to prevent any shake after the length of zinc necessary for the compensation shall have been accurately determined.

In the two opposite sides of the zinc tube two small grooves (*a*, *b*, fig 3) are made with a file at about the eighth of an inch from the top of the tube, and parallel to it, and about the twentieth of an inch in depth.

A piece

A piece of iron, is forged one foot long, half of which is made of the same size as the zinc tube is *externally*, and the remainder of the same dimensions as the *upper part* of the pendulum rod. This iron being placed between the plates of brass, which form the weight of the pendulum, the lead is cast around it, and the iron is afterward forced out, leaving a receptacle for the tube of zinc, which extends *exactly* to the centre of the weight. This compensation described.

The brass plate at the back of the weight of the pendulum being taken off, a square opening (the upper part of which is in a line with the centre of the weight,) is cut through the lead into the receptacle for the tube of zinc; and the tube having been made to slide with perfect freedom, yet without any shake in the receptacle, it is passed up as far as it will go; and marks being made opposite the groves *a*, *b*, fig. 3, as represented at fig. 4, corresponding groves are made in the lead with a gouge of the proper size, into which a small quantity of melted lead being poured, the tube of zinc remains firmly secured, and of course can expand in no other direction than from the centre of the weight downwards.

In order to restore the lead, which was taken away in making the square opening; a piece of card paper is cut so as to cover the zinc, and the opening may then be filled with melted lead (being careful that it is not too hot,) without any danger of its uniting with the tube: the back plate of the pendulum weight is then to be replaced and rivetted.

To the lower end of the pendulum rod a cap of brass is firmly fixed, from the bottom of which a strong steel screw proceeds, which passing through the hole in the crylinder of zinc, the weight is supported, and the pendulum regulated, by a nut in the usual manner; and a small octagonal plate of brass is soldered to the bottom of the cylinder, to prevent the nut from injuring the zinc, as well as to divide each revolution of the screw into eight equal parts.

To determine the length of zinc required for a compensation of this form, that part of the steel screw included between the nut and the end of the deal rod must be considered. This we will suppose to be two inches, which will leave half an inch, by which the length of the pendulum may be varied: Determination of the length of zinc for this compensation.

ried: adding this to one inch, the length of the spring by which the pedulum is suspended, we have three inches of steel; and the expansion of steel to zinc being as 147 to 353, we have $\frac{147 \times 3}{353} = 1{\cdot}25$ inches nearly for this part of the correction.

The deal rod will be about 44·5 inches long; and its expansion being to that of zinc as 49 to 353, we have $\frac{49 \times 44{\cdot}5}{353} = 6{\cdot}17$ inches nearly for the length of zinc necessary to counteract the expansion of the deal, which being added to 1·25 inches, before found, gives 7·42 inches for the whole compensation sought.

Mode of adjustment.

The adjustment is effected in the same manner as before described by means of the screw E F, by which the length of the zinc is either increased or diminished; and below the large weight is a smaller one, for the purpose of regulating the pendulum to the greatest nicety. This small weight may have a tube of zinc attached to it, on the same principle as that of the larger, to correct the expansion of the steel screw, if it be thought necessary.

Objection to this compensation.

The chief objection to this pendulum appears to be, that the compensation is partly enclosed in the weight, and consequently is not likely to be so soon affected by any sudden variation of temperature, as it would be if it were exposed to the immediate influence of the atmosphere. But it has the advantage of being much shorter, and far more simple in its construction, than the one first described, and is therefore on the whole perhaps preferable.

Its advantages.

The compensation may be divided.

If it be thought more convenient, the compensation may be divided, and half placed between the weight of the pendulum, and the other half on the cock of the time-piece; and the nut for regulating it may be either above or below.

Advantages of this pendulum.

Experiments in regular observatories can alone determine the relative merits of this pendulum; It certainly possesses the superior advantages of economy, simplicity, and ease of adjustment, and there appears every reason to believe, that it may be found at least equal in point of accuracy to any that has hitherto been described.

Exeter, April, 1808.

XI

XI.

Extract of a Letter from Mr. J. Aston of Ipswich, giving an Account of a Mule Cucumber, and other Objects.

I Have taken the liberty of sending you a curious production of nature, which was produced in the following manner; Mr. Chapman, the proprietor of very extensive pineries in this town, had growing in one of his hot houses a plant of the cucumis colocynthis (coloquintida, or bitter apple), which happened to put forth a male blossom a day or two before it was removed into the open air. In the same house there were also growing some plants of the common cucumber also in blossom at some distance from the other plant. It is supposed some of the farina was carried by a bee from the blossom of the coloquintida to a female one of those on the cucumber, which thus became impregnated, and produced the fruit I send you. Mr. Chapman says he noticed the cucumber when about an inch and a half or two inches long, and it had every appearance of becoming a very fine fruit, but soon afterward it began to swell, and continued to do so till the other day, when he gathered it and presented it to me.

Curious natural production.

Farina of the coloquintida impregnated a common cucumber.

I had some thoughts of sending you a drawing of it, but, as I am a very indifferent botanist, it struck me, that you would not be able so well to understand its nature either by delineation or description, as by seeing the fruit itself. Mr. Chapman is an intelligent man, and has been for many years engaged in horticultural pursuits*. You will perceive it is what is called a mule fruit partaking of the nature of both the parent plants. See Pl. V, fig. 5.

If you should be of opinion, that it is a circumstance worth mentioning in your Journal, I beg you will do it in any manner you please, and in a way that you think will be most easily comprehended by botanists, to whom most probably the communication will be found acceptable.

* This Effect, I am informed is not unfrequent, and is ascribed to bees. Whole beds of melons have in some instances been thus spoiled. N.

When

Pyrites on the shore at Harwich.

When I was in London you may perhaps recollect I mentioned to you, that considerable quantities of iron pyrites were to be found upon the sea shore at Harwich. I have embraced the present opportunity of sending you a small specimen for your inspection. It would be curious to ascertain the true theory of its formation. From the little observation I have had an opportunity of making, I am persuaded its formation is considerably aided by the seawater. Pieces of wood, bone, &c. become converted into it by time, and lose every trait of their origin, except the shape of the grain, which in many specimens is nicely preserved. The cliff above the shore appears to be almost entirely composed of a blueish soft clay, which is continually crumbling and falling down upon the beach, and is washed by the waves, and I think a curious observer conversant in mineralogy might easily trace the formation of the pyrites by gradation from the clay, as pieces may be found in several different states, and it appears to be influenced by the alternate action of the air and sea water, but in what way I am entirely at a loss at present to conjecture.

Seawater appears to assist in its formation, converting wood, &c. into pyrites.

Ipswich, 6th of June, 1808.

XII.

Letter from Professor VINCE, *in Reply to* DYTISCUS.

To Mr. NICHOLSON.

SIR,

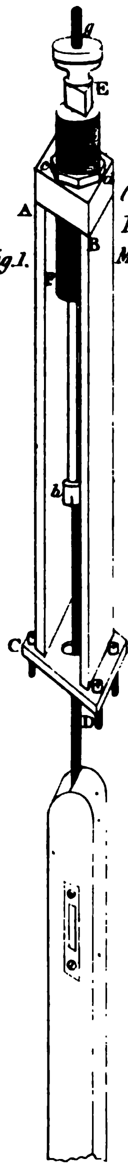
Two mistatements in the last letter of Dytiscus.

I Shall esteem it a favour, if you will insert a few remarks on the observations of Dytiscus in your last Journal, engaging not to trouble you again on this subject.

In the first paragraph there are two *unaccountable* misrepresentations, for I would not charge Dytiscus with doing it wilfully. He says, "the two first terms of the series very possibly allude to the two first terms of the *only* TWO series which are to be found in the essay, these two terms having been already mentioned *as sufficient for determining the force*." Now I have put down the first terms of THREE of the

Nicholson's Philos. Journal Vol. XX Pl. 5 p...

Fig.1.



Compensation Pendulum by Mr. Henry Kater.

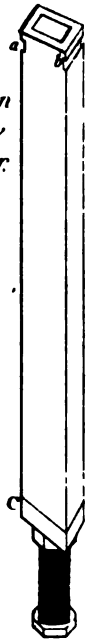
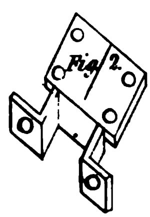
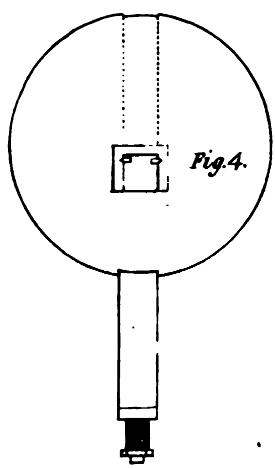


Fig. 3.

Irregular production of the Cucumber.





the series, with + &c., meaning, of course, that the other series and terms were to be supplied, as I before remarked, and which it is strange Mr. D. should have forgotten. And secondly, I have never mentioned, nor was it possible I could mention, that the two terms alluded to are sufficient to determine the force: a further proof with what little attention Dytiscus has read the essay.

Again, he says, "the series $\frac{\alpha}{a^2}+\frac{\beta}{a^4}+\frac{\gamma}{a^6}+$ &c. may *certainly* vary as $\frac{1}{a^2}$, if all the Greek letters after the first become inconsiderable, and our author has virtually confessed in his essay, that they do become inconsiderable." The series certainly can *not* vary as $\frac{1}{a^2}$. The quantities β, γ, &c. are very small, but still finite, and can only be rejected in an *approximation* to the law of force. The law of gravity varies *accurately* as $\frac{1}{a^2}$, and the series can never give that law, as I have proved in Art. 11. An assertion of his erroneous.

Farther: "As to the difficulty of extending the law to the internal parts of the sun's substance, *it is perfectly obvious*, that the law of density, as well as that of the force, must be supposed to change at the surface of every material body, long before $\frac{Q}{a}$ can become equal to P." *Not* perfectly obvious. When we discover *sudden* variations of the laws of nature, it is not that the *primary* cause is necessarily altered, but that some of the circumstances under which it acts are changed, as in the present instance. Without considering the cause, we know, that the attractions of every two particles of matter composing the sun's body vary inversely as the squares of their distances, and at the same time constitute a whole force, which, to a body *external* to the sun, varies as $\frac{1}{a^2}$, and to an *internal* body, as a. It is not therefore necessary, that the law of attraction of the constituent particles should vary, in order to produce these different laws of force. According to Newton, *any* two particles Another contradicted.

ticles of matter, either both within a body, or one within and the other without, tend toward each other by the same law of force, and therefore the *cause* of that tendency, that is, in our present consideration, the *variation* of the density of the fluid, must in both cases be regulated by the same law. If we were to admit the position advanced by Dytiscus, it would involve Newton in a contradiction, and instead of affecting the truth of my proposition, would further tend to confirm it. To change the law of density *immediately*, would be to substitute two fluids instead of one, such a change necessarily implying a change of the fluid; for what better criterion have we of different fluids, than that their constitutions are regulated by different laws? To defend his objections, Dytiscus makes an assumption totally inconsistent with Newton's hypothesis.

which would involve Newton in a contradiction.

I do not think it necessary to make any farther remarks on the observations of Dytiscus, and I must make an apology to mathematicians for having said so much; but I was induced to do it upon this consideration, that they might not mislead those who are ignorant of the subjeet.

I am, Sir,

Your obliged humble servant,

Cambridge, 9 *June*, 1808. S. VINCE.

XIII.

Certain Improvements in Chronometers, by DANIEL DERING MATHEW, *Caius College, Cambridge. In a Letter from the Author.*

SIR,

Chronometers have been greatly improved.

THE degree of accuracy, to which chronometers have been brought within these few years, may appear to be the utmost to which, in a machine so complicated, human art could extend; but as navigation has derived great advantages from improvements made in them, I have been tempted to make some alterations in their construction, the superiority

superiority of which I leave for your candid readers to decide.

The principle of Mr. Mudge's free escapement (see 4to Journal, vol. ii, p. 56) is, I believe, allowed to be the best that was ever offered to the public; but its performance has not been found to be superior to the others, most likely on account of there being so many pivots and springs, and on account of its tripping, whence it cannot be depended upon. Mr. Arnold says, he has made his pendulum spring so, that the vibrations are performed in the same time when the main spring is weak, as when it is strong. This perhaps may be in some degree accomplished by very fine workmanship, and a great many trials, but the main spring is not detached from the balance; and on this account I think the title of being detached is not correct, as the main spring keeps up the action of the balance.

Mudge's scapement.

Arnold's.

My alterations and improvements, if I may so call them, consist, 1st, In reducing the wear and friction of Mr. Mudge's escapement, and putting it into a more simple form. 2dly, In applying my equalizing maintaining powers in such a manner, that tension does not alter their strength. 3dly, In securing the locking of the tooth against the detent. 4thly, In stopping the holes with hard platina.

Principles of the author's.

For these purposes I have two escapement wheels, equal and similar in all respects, as seen in Pl. VI, fig. 1. *a* A, *b* B, *c* C, *d* D, *e* E, *f* F, represent the teeth of the two wheels, which are so placed, that the tooth A of the upper wheel is exactly between the two teeth *a b* of the lower wheel. These teeth are prevented from revolving round by the two detent pallets G H, which turn on a pin, and concentric with these detent pallets the pivots of the verge turn, which is in the form of a crank as at M, or more plainly at fig. 2. *y y* are two joints at the ends of each of the arms of the pallets G H, in which the pieces *x x* are screwed, so as to allow a free motion. These pieces are fixed to the ends of two springs K L, which are made similar to the main spring in a gun lock. Each of these springs turns upon a stud *m*, as seen at K, fig. 3; and the spring is made stronger or weaker by the regulating screw *n*. The stud *m* is made of brass and the screw is steel, therefore the

Description of it.

the greater expansion of the stud will in some measure counteract the alterations of the springs by heat and cold. N N is the potance.

Its manner of acting.

The action of the escapement is thus. The wheel being propelled by the main spring in the direction of the arrow V, and prevented from revolving by the detent of the pallet H, the balance by its vibration knocks out the detent at *d*, and at the same time the tooth E raises the pallet G, till it comes to its detent. In the mean time the balance carries the pallet H through its semivibration, and is followed back by the pallet as far as the rim of the wheels between the teeth *d e*, this gives the balance force sufficient to knock out the detent of the pallet G, and the same action and reaction will continue so long as the moving power acts.

To prevent any possibility of the wheel tripping, I put two banking pins as at *g* on the arm of each pallet, which prevent the pallet from going farther back than is necessary to allow the tooth to raise up the pallet to its detent, by means of the catches *p p*, the end of which is a fine tender spring; and I make a circular piece to project out from the catch, so that the crank in its vibration first raises up this catch, and keeps it up while it knocks out the detent.

Advantages of this scapement.

Having explained the action of my scapement, I will now state a few of the advantages, which appear to me to arise from these alterations. 1st. By making use of a double wheel, I not only reduce the wear of the teeth, but I can in this way place my detent pallets and back springs so as not to interfere with one another, and I can have the pallets to turn upon the same centre. 2d. Straight springs are always preferable to a spiral one, where they can be used, because they are not so difficult to make, and their strength can be altered by adjusting screws, which cannot be done when spiral springs are used. Another advantage gained by using straight springs is, that the compensation may be put to the springs themselves, which is preferable to a compensation on the balance.

Objections answered.

In using a gun lock spring, the pressure of the tooth against the inclined plane is equal, and will therefore wear the face of the pallet equally. I am aware, that many objections

objections will be made to these springs on account of there being so many joints; but as there must be either the rub of the spring up and down the back of the pallet, or a double joint: the latter method is certainly preferable to the former.

In Mr. Mudge's watches, the adjusting of his auxiliary spring to prevent tripping was one reason, why they frequently stopped; for when clean, the main spring was adjusted just to raise up the pallets to their detent, and therefore, when the oil got more tenacious, and the works got dirty, the main spring had not power to raise the pallet; the consequence of which was, the watch stopped. Defect in Mudge's.

The pendulum spring is generally allowed by workmen to be the most difficult part of a chronometer to make and adjust well. The two back springs answer the purpose of the regulating power, as well as the maintaining power. Pendulum spring.

As platina is the closest grained metal we have, and it can be drawn very hard, I prefer stopping the holes with it. It burnishes very fine, and oil has no chemical action on it. Preference of platina.

If you think these improvements worthy a place in your Philosophical Journal, by inserting them you will oblige

Your sincere friend,

DAN. DERING MATHEW.

XIV.

Observations on the Possibility of collecting a certain Quantity of Succinic Acid, during the Preparation of Amber Varnish, without any Injury to the Quality of the Varnish: by Mr. Planche, *of the Society of Apothecaries, Paris**.

HAVING had occasion lately to assist in the fabrication of a large quantity of amber varnish, I remarked, that during the process, and till the heated substance had ac- Succinic acid sublimes in making amber varnish.

* Annales de Chimie, vol. XLIX, p. 40.

quired the proper degree of fluidity, a great deal of succinic acid was given out.

No advantage has been made of this.

Every person, who has made it, must have had the opportunity of observing the same thing; but whether from not knowing the true nature and properties of this salt, or from considering it as essential to the goodness of the varnish, no one, at least that I know of, has thought of turning it to advantage. It would be a mistake however to conclude, that good varnish ought to be free from succinic acid: on the contrary it is very probable, that at the time when the drying oil and oil of turpentine are added, to increase the fluidity of the amber, this substance is still capable of furnishing it, and even in some quantity.

Amber in varnish should not be totally deprived of its acid.

Much of the acid lost in the common way of making.

I should be wandering from the purpose of this notice, if I were to detail the various processes employed for the preparation of this varnish. I shall only say, that, as the process is most commonly conducted on an open fire, and in an open glazed earthen vessel, the mouth of which is four or five inches in diameter, when the matter is sufficiently heated, part of the acid set free is carried off and lost in the air, while a tolerable quantity adheres to the sides of the matrass in the form of very slender needles, sufficiently white to require no purification *.

1½lb. of amber will yield 80 or 90 grs. of acid without detriment.

Every matrass containing 24 oz., which is the common quantity, may furnish 80 or 90 grains of acid, without any injury to the quality of the varnish: a fact of which I have satisfied myself by several trials made in my own laboratory, as well as in that of Mr. Tonnelier, coach painter, who is well skilled in the subject. It is proper to observe here, that we ought to collect the succinic acid as it is sublimed, which takes place a little before the addition of the oxigenized or drying oil. If this operation were deferred, the greater part of it would be lost. In fact, the motion of the spatula necessary to mix the oil with the amber would separate a great deal of the acid: and there is no

Proper time of collecting it.

Acid pure in a new vessel.

Copper best.

* The acid obtained is sufficiently pure when the vessel is new, but it is more coloured in subsequent operations. It may then be purified according to the method indicated by Pott. The artists who use copper matrasses will find an advantage in it, for these vessels being more easily cleaned, they will continue to furnish the same product.

hope

Mr. Mathew's Scapement.

Fig. 1.

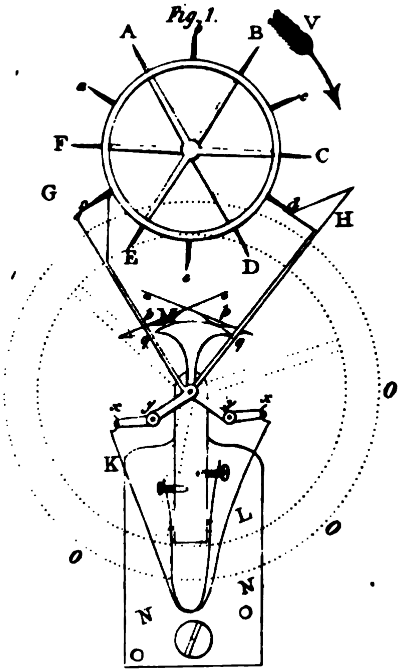


Fig. 2.

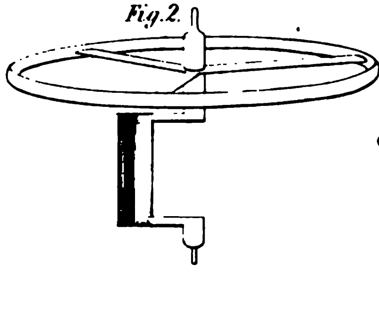
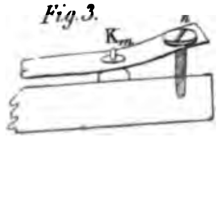


Fig. 3.



Planche's Apparatus for Succinic Acid.

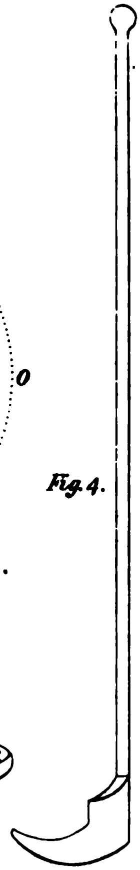


Fig. 4.

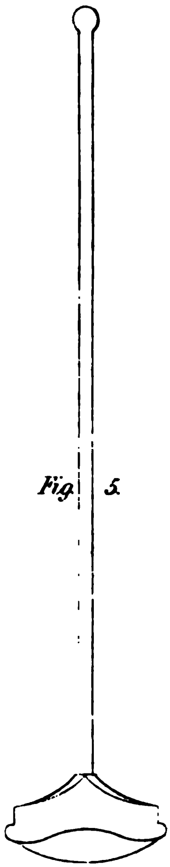
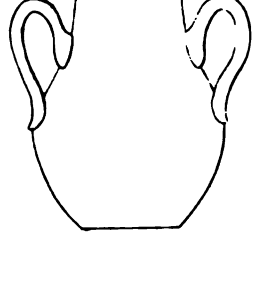


Fig. 5.

Fig. 6.



hope of collecting any after the oil of turpentine is added, as this oil, partly converted into vapour by the heat of the mixture, so as to make it swell up or even boil over, occasions the acid to disappear entirely.

However minute the means I have employed for collecting the succinic acid may appear, I think it indispensable to describe them. At first I thought of taking it off with a card. This answered pretty well, but there is danger of burning the fingers, if from inattention they should touch the heated matter. I found a much more convenient instrument was a tin spoon, made as represented in Pl. VI, where fig. 4 is a side view of it, and fig. 5 a front view. This spoon differs from others only in the form of its bowl, which is but little concave, the front of it forming a segment of a circle, and adapted to the size of the matrass; which is represented at fig. 6, but on a much smaller scale, not to occupy too much room in the plate. The bowl of the spoon is terminated behind by a thin plate of iron, which rising a few lines above its edges forms a sort of neck, and to this is joined a handle of the same material, sixteen inches long, forming a right angle with the bowl. The shape of this spoon appeared to me the most convenient, because, 1st, as it adopts itself accurately to the sides of the vessel, it prevents the sublimed acid, which is scraped off by drawing up the spoon, from mixing with the melted amber: and 2dly, it allows the operator to collect it without being incommoded by the vapours emitted.

Method of doing this.

Instrument described.

From what has been said it appears, that artists employed in making amber varnish, without any alteration in their usual processes or apparatus, may furnish us in future with a pretty large quantity of succinic acid, which has hitherto been confined to medical uses, but may soon be found beneficial in other arts. Some trials already give me room to hope, that its solution in alcohol may be employed to imitate the colour of some valuable woods.

Varnish makers may collect a considerable quantity of the acid.

Useful for imitating some fine woods.

XV.

XV.

An Essay on the Saccharine Diabetes; by Messrs. DUPUYTREN *and* THENARD. *Abridged by the Authors.*

Urine changed in diabetes,

IT has long been known, that the nature of the urine is so much changed in the disorder called diabetes, as, instead of being pungent and in small quantity, like that of a healthy person, it is on the contrary saccharine and very copious.

but its analysis of recent date.

The first attempts at its analysis however are not to be dated farther back than thirty years. For this three reasons may be assigned: first the rarity of the disease; secondly the little certainty of the chemical means of analysis formerly employed; and thirdly the common neglect of animal chemistry.

Sugar demonstrated in it.

It was not till 1778 that the existence of sugar in diabetic urine was actually demonstrated. This discovery, made by Caulcy, and confirmed in 1791 by Franck, was conjectured by Willis in the beginning of the 17th century, and in some measure perceived by Poole and Dobson. But it must be confessed, that Caulcy, attending only to the saccharine matter of this sort of urine, left much to be desired. It was necessary to inquire into the other principles it might contain, and particularly those that enter into the composition of healthy urine. This was done in 1803 by Messrs. Nicolas and Quendeville of Caen. From their researches it appears, that diabetic urine contains no sensible portion of uree or of lithic acid; that the most sensible tests scarcely indicate any traces of phosphate or sulphate; that it is impossible to discover in it any free acid; and lastly that we find in it only a large quantity of sugar with more or less common salt.

General state of it in the disorder.

Objects of the authors.

Our object in this essay is not merely to confirm the results we have mentioned, but farther to make known

1. The medical observations we have made on the patient, whose urine we analysed:

* Annales de Chemie, vol. LIX, p. 41.

2. The

2. The very peculiar nature of the saccharine substance we found in this urine: and

3. The various changes this urine underwent before it was brought back to its primitive composition.

PART I. *Observations made on the patient, whose urine we examined.*

Observations on the disease.

From these observations it follows: 1. That the saccharine diabetes may continue several years, and even as long as the digestive powers can maintain themselves, and supply the excessive waste occasioned by the urine.

May continue several years.

2. That this disease is not incurable at any period, not even when the impaired digestion appears unable to supply the materials of the secretion that exhaust the animal economy.

Curable at any period.

3. That the seat of this affection appears to be in the kidneys, not in the intestinal canal.

Its seat the kidneys.

In fact neither the appetite nor thirst of a diabetic patient is any way depraved: they both, as well as the digestive powers, appear merely to be proportional to the want of reparation; in the next place the aliment undergoes the same preparation in the stomach of a diabetic patient as in that of a man in health; and what completely proves, that the digestive faculty is not altered, but simply increased, in diabetic patients, is the quantity of food they take, the quickness with which it is digested, the large proportion of it conveyed into the circulation, and the small quantity of fæces to which it is reduced; and lastly, from the digestion of the food till the secretion of the urine, we find no fluid at all saccharine, or that has undergone any change in its composition.

4. That the cause of the saccharine diabetes appears to be an increased and depraved action of the kidneys; that the saccharine matter of the urine is produced in consequence of this action; and that to it all the symptoms of the disease are to be traced.

Its cause their increased and vitiated action.

5. That the excessive loss, which takes place in this disease, seems under some circumstances to occasion a pretty considerable absorption at the surface of the body.*

Superficial absorption increased.

6. That the new proportions established by the saccharine diabetes

Secretions af-

fected as by evacuations in general.

diabetes between the food and the secretions in general, and between their several kinds in particular, are analogous to those occasioned by any evacuation in excess, whatever its nature may be.

Dr. Rollo's treatment infallible.

7. That the mode of treatment recommended by Dr. Rollo, and since so successfully employed by our countrymen, Messrs. Nicolas and Quendeville, and which consists especially in a purely animal diet, is as effectual as the bark in intermittent fevers.

Does not alter the state of the organs.

8. Lastly, that the saccharine diabetes produces no change in the state of the organs, but an exertion of the digestive and urinary organs, both of which are in a state of great activity during this disease, one to prepare and the other to expend the materials of nutrition.

Analysis of the urine.

PART II. *Analysis of the urine of a diabetic patient from the fifteenth day after his admission into the Hôtel Dieu, till he left that place for the hospital of the Medical School.*

Its appearance.

This urine, very remarkable for the largeness of its quantity, emitted a smell, that was not disagreeable. It was limpid, perceptibly yellow, of greater specific gravity than water, and scarcely reddened infusion of litmus. Its taste was slightly saccharine, and at the same time it had something of that of common salt.

Change by keeping.

Left to itself at the temperature of 15° [59° F.], it became turbid in five or six days: bubbles of carbonic acid gas were disengaged on the slightest agitation: the urinous smell it had at first was gone, and it had acquired a smell resembling that of newly made wine: it likewise afforded alcohol by distillation, and became very sour by exposure to the air, so that it exhibited in a slight degree all the marks of a spirituous fermentation.

Distilled.

Distilled in a retort, or evaporated in a capsule, the phenomena it exhibited were the same. It did not become turbid, gradually thickened, and was reduced to a sirup, which sometimes amounted to a seventeenth, sometimes to a twentieth, and never to less than a thirtieth of its weight. From the urine we examined we thus obtained near thirty pounds of sirup, which on cooling always dried into a mass, composed

posed of a multitude of small grains void of consistency. These soft granulous crystals being scarcely sweet, it was natural to suppose, that the substance which formed them was not homogeneal, and included but a very small quantity of the saccharine principle. To ascertain this the following experiments were made.

Saccharine matter distilled.

A hundred parts of this substance were distilled in a retort, the neck of which entered into a receiver kept constantly cool. The products were a great deal of water, but little oil, no ammonia, a larger quantity of gasses that were but slightly fetid, and a tolerably bulky coal, easy to incinerate, and when completely incinerated yielding two parts and half of common salt, and half a part of phosphate of lime.

General conclusions.

From this result we may deduce the following consequences: 1. that this substance contained no animal matter, since it yielded no volatile alkali on calcination: 2. that it contained very little saline matter, since when reduced to ashes it afforded only a residuum equal to a few hundredths of its weight: 3. that it was formed of vegetable principles, since it afforded all their products on distillation.

Fermented with water and yeast.

Presuming sugar to be one of these principles, and not being able to form any conjecture respecting the nature of those with which we considered it to be mixed, we determined to have recourse to fermentation, to destroy the first without altering the others, so that by filtration and evaporation we might obtain them very pure. We put into a large jar 100 gram. [1544·5 grs.] of the substance to be analysed, 25 gr. [386·125 grs.] of yeast, and 500 gr. [7722·5 grs.] of water: to the tubulure of this jar we fitted a tube terminating under a jar filled with water: and the temperature being raised to 18° [64·4° F.], the whole was left to itself. Some hours after these matters had been thus left together, a movement was apparent in some parts of the fluid, which soon became general. A great deal of flocculent matter, from which a considerable number of bubbles issued, was raised up, and carried to some height in the fluid. These bubbles passed rapidly into the jars filled with water, but the flocks fell to the bottom of the vessel, and, giving birth to new bubbles, rose again, to be precipitated

Fermentation active. tated as before. This phenomenon, which did not cease for three days, indicated a very active fermentation, and consequently the presence of a large quantity of saccharine principle. Carbonic acid evolved, In fact near thirteen quarts of pure carbonic acid gas were evolved: the liquor was very spirituous, and contained near 48 parts of alcohol at 40°: alcohol produced, and on evaporating to dryness only 23 parts of extract were obtained, formed of and a residuum left. 3 parts of seasalt, and 20 parts of a brown viscous matter.

Resembled sugar in its products, Now we know, that 100 gr. [1544·5 grs.] of sugar produce 12 gr. [185·34 grs.] of a similar residuum, 56 gr. [864·92 grs.] of alcohol, and 36 gr. [556·02 grs.] of carbonic acid. The substance obtained from diabetic urine therefore gave us by fermentation the same products, and nearly in as large quantity, as the best crystallized pure sugar: and its habits with reagents. and if to this we add, that with nitric acid, alcohol, and other reagents, it comports itself like sugar, we must necessarily consider these two substances as being in some measure identical.

Yet differs from sugar in taste. We must recollect however, that it is scarcely sweet, and that at any rate it is much less so than sugar. Hence we Different species of sugar. are led to conclude: 1. that, as chemists have lately begun to imagine, there are different species or varieties of sugar: for here the differences are so striking, that they must convert to a certainty what was only probable. But as the taste is not a certain indication of the existence of the saccharine principle, it became necessary to inquire, whether, among the substances that have hitherto been confounded with sugar on account of their taste, there were not some, that differed from it essentially. Manna fermented with yeast and water. We were thus led to examine manna. Our first care was to mix it with yeast and water at the temperature of 18° [64·4° F.], and observe with attention all the phenomena arising from this mixture. Fermentation brisk, but soon over. The fermentation quickly took place: it was at first brisk, but soon abated: and at the expiration of two days it was at at end. Left a sweet matter incapable of fermenting. The liquor however had a very strong vinous smell; but, far from being spirituous, it was on the contrary very saccharine; and on evaporation it deposited in the form of crystals almost all the matter that had been employed, divested of the faculty of fermenting.

This matter examined. Though persuaded by these results, that manna contained but

but a very small quantity of sugar, we still deemed it necessary, to compare it with this substance in all its properties, in order to place the fact in the strongest light, and thus discover all the characters proper to the peculiar principle, of which it appears to be almost wholly formed. For this reason we examined the action of alcohol on it, which does not attack the saccharine principle, and that of nitric acid, which does not convert any portion of this principle into mucous acid. The first of these reagents, at the temperature of 60° [140° F.] dissolved so large a quantity of manna, that on cooling it formed a mass of crystals in groups, the crystals in each group issuing from a common centre. The second produced in it by long continued boiling such a large deposit of mucous acid, that the weight was nearly equal to half that of the manna employed.

Hot alcohol took up so much as to become solid on cooling.

Nitric acid converted half to mucous acid.

Here then we have two more characters, that strikingly distinguish sugar properly so called from the peculiar principle of manna.

These characters distinguish manna from sugar,

No doubt farther research would exhibit many others more or less striking; but as those we have related are sufficient, to make those two substances be considered as perfectly distinct from each other, we did not think it necessary to push our examination farther.

and perhaps others.

Hence it follows, that it will always be an easy matter to discover and to separate manna, or rather the peculiar principle of manna, whatever be the substances with which it is mingled. All that is necessary is to treat the matter containing it with hot alcohol, and it will be almost entirely precipitated by cooling. Indeed there are other vegetable substances, that possess this property even in a striking degree; but as these substances are found only in this class of acids, it is always practicable to deprive it of these, by combining it with an alkaline or earthy salifiable base, or a metallic oxide, according to the nature of the acid; and consequently this mode of separation may be generally employed.

This principle of manna separable by alcohol.

Thus we may ascertain, whether the honeydew observed on the leaves of certain trees, particularly those of the lime, be really a species of manna; and if it be the same with the saccharine principle that exists in asparagus, and which

Honeydew?

and saccharine principle in asparagus?

Messrs.

Messrs. Vauquelin and Robiquet have found there mixed with a peculiar principle.

PART III. *Analysis of the urine of the diabetic patient, from the time of his admission into the hospital of the Medical School till he quitted it.*

Medicine without regimen had no effect on the urine.

During the time the patient was in the Hôtel-Dieu, he could not be confined to any regimen. He lived nearly as he pleased; his disorder remained stationary, and his urine, which was still very abundant, had not altered its nature. It was then determined, to remove him to the hospital of the Medical School, where, being almost always under the eye of Mr. Dupuytren, who had the care of him, or of some one of his pupils, it was much more easy, to oblige him to do whatever was desired.

Vegetable food being withheld

At the expiration of a few days all kinds of vegetables were refused him, and nothing was given him but animal food. The quantity of this he took, as well as of what he drank to satisfy an unquenchable thirst, was accurately weighed.

in a few days a change took place.

For the first three or four days no change in the urine was observed; but in five or six it was less white, more acrid, more acid, and less saccharine. Subjected to evaporation, instead of remaining limpid as before, it became turbid, and was covered with a tolerably thick pellicle of albuminous matter. When I perceived this change, particularly the presence of animal matter in his urine, though the state of the patient was completely unknown to me, and I was unacquainted with the manner in which he had been treated, I concluded, that the disorder had begun to abate: and then finding, that this animal matter became daily more abundant, I considered the cure as approaching. Mentioning my opinion to Mr. Dupuytren, he said it was probable, but appeared surprised at it, till I informed him on what it was founded.

Albumen appeared in it, and increased in quantity.

From that time the patient continued to amend. His urine grew daily more animalized, and less saccharine. The albuminous animal matter soon began to diminish gradually, and the uree and lithic acid as gradually reappeared. At length it became perfectly similar to that of a man

The albumen diminished, & uree and lithic acid began to appear.

man in health, and the patient was cured. Immediately on this however he indulged in excesses of various kinds, when the diabetes returned, complicated with other disorders, under which he soon sunk.

Excesses brought on a fatal relapse.

If now we take a review of all the inductions that may be made from the experiments just related in the second and third part of our memoir, we may affirm

General conclusions.

1. That the diabetic urine we examined was composed almost wholly of a substance but little saccharine: and that nevertheless it possesses all the properties that characterize sugar; for it is converted into alcohol and carbonic acid by fermentation, affords a great deal of oxalic acid and no mucous acid when treated with nitric acid, is very little soluble in alcohol at 36°, and produces when calcined but little oil, and a great deal of water and carbonic acid. And thus it is demonstrated, that there are different varieties of sugar.

State of the urine.

2. That manna is not a species of sugar: that it contains but a small quantity, which may be destroyed by fermentation: and that, on the contrary, it contains a great deal of a peculiar principle, the taste of which is very sweet, and the chief characteristics of which are not to ferment with yeast, to yield a great deal of mucous acid with nitric acid, and to be more soluble in hot than cold water, but particularly in alcohol, so that the solution on cooling becomes a crystalline mass.

Manna not sugar.

3. That if nothing but animalized food be given to diabetic patients, their urine changes its nature pretty quickly: that at first we find in it an albuminous matter; that this albuminous matter the quantity of which continues increasing for some days, appears to be an unequivocal sign of a cure: that afterward the albumen gradually disappears: that the kidneys then begin to secrete uree, lithic acid, and no doubt acetous acid also: and that the urine soon becomes similar to that of a person in health: but that nevertheless, to prevent a relapse, the patient ought still to continue his regimen of an animal diet for a considerable time, and take nothing that might bring on the diabetes afresh.

An animal diet will cure,

but must be continued long to prevent relapse.

XVI.

XVI.

Letter from Mr. Roloff, *of Magdebourgh, on the fetid Resin of Sulphur**.

Fetid resin of sulphur obtained in making golden sulphur of antimony.

I lately had an opportunity of detecting Mr. Westrumb's fetid resin of sulphur † in an unexpected manner.

Mr. Michaelis, after having precipitated the golden sulphur of antimony from the hidroguretted sulphuret of antimoniated potash by means of sulphuric acid, evaporated the supernatant liquor, which held the sulphate of potash in solution.

Heated smells like burning asafœtida.

When the solution began to be concentrated, a vapour arose, by which the artist who was stirring it was singularly incommoded. At the same time an insufferable stench was emitted, resembling that of burning asafœtida.

The saline mass, being evaporated to dryness, was of a gray colour, and had the remarkable smell just mentioned.

Gives to alcohol the taste & smell of garlic.

Being digested with alcohol, it imparted to it the smell and taste of garlic.

The alcoholic solution, left to evaporate spontaneously, yielded a gray, glutinous mass, having a similar taste and smell.

Easily procurable in quantity.

I was desirous to impart the knowledge of this fact, as I know not whether Mr. Westrumb be acquainted with the formation of a large quantity of the fetid resin, which may easily be procured by this process.

Formed independently of alcohol.

Since the smell displays itself before any alcohol is added, we may conclude with Mr. Westrumb, that the alcohol does not contribute to its formation.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Wernerian Society.

At the last meeting of the Wernerian Natural History Society, June the 11th, Dr Thomas Thompson, one of the Vice-Presidents, read a very interesting and valuable paper on the chemical nature of fluor-spar. Captain Laskey also read a paper on the pinna ingens of Pennant: from his observations, it appears, that the pinna ingens of Montague, pinna corealis of Stewart, and pinna ingens of the Linnean Transactions, are the same species, and identical with the pinna ingens of Pennant. At the same meeting, Charles Anderson, Esq., read some observations on the geognosy of the island of Inch Keith, in the Frith of Forth. It appears from the interesting details which he communicated, that the whole island is composed of rocks belonging to the independent coal formation; and that the greenstone, which there occurs, is traversed by true veins filled with quartz, chalcedony, calcspar, &c, and also contains numerous contemporaneous veins of different kinds. Mr. Anderson intimated his intention of laying before the Society, at a future meeting, a more particular description of the island, illustrated by drawings and a series of specimens.

Pinna ingens.

Geognosy of Inch-Keith.

* Extracted from Gehlen's new Chemical Journal. *Annales de Chimie*, vol. LXII, p. 190. † See Journal, vol. XVIII, p. 41.

TWO

TWO METEOROLOGICAL TABLES for 1807,

Communicated by Dr. CLARKE, of Nottingham.

QUANTITY OF RAIN,

WHICH FELL AT THE FOLLOWING PLACES IN THE YEAR 1807,

In Inches and Decimals.

By the REV. J. BLANCHARD, NOTTINGHAM, who solicits Communications.

1807.	*Chichester.*	*London.*	*Diss, Norfolk.*	*Chatsworth, Derbyshire.*	*Horncastle, Lincolnshire.*	*Ferriby, Kingston-upon-Hull*	*Heath, near Wakefield, Yks*	*Lancaster.*	*Dalton, Lancashire.*	*Kendal.*	*Sedbergh, Yorkshire.*	*Nottingham.*
JANUARY, .	2·41	0·64	1·67	1·40	1·50	0·82	0·85	3·55	2·78	2·92	2·38	0·75
FEBRUARY,	2·44	1·48	1·66	1·79	2·77	2·64	2·69	3·59	4·59	5·58	4·00	1·23
MARCH, ...	0·23	0·50	1·56	0·44	1·69	1·21	2·60	1·12	1·52	2·31	0·57	0·73
APRIL, ...	0·00	1·02	0·81	0·67	2·36	1·77	1·17	3·19	5·66	2·90	1·72	0·94
MAY, ...	5·47	3·26	3·47	5·26	2·80	2·53	4·70	3·75	3·97	4·47	2·86	5·08
JUNE, ...	0·56	1·74	1·92	2·81	2·32	1·13	2·65	1·25	2·26	2·27	4·00	3·00
JULY, ...	1·62	0·38	1·54	2·26	2·25	1·11	2·45	3·50	3·74	4·48	3·43	2·55
AUGUST, ...	3·13	1·94	1·64	1·57	1·27	3·31	2·18		2·92	3·49	4·58	1·40
SEPTEMBER	3·22	2·18	2·17	1·27	1·45	2·86	3·34	} 10·08	10·27	7·92	6·86	1·70
OCTOBER,..	2·48	0·94	0·90	3·15	1·78	1·93	1·60		6·08	7·09	5·15	1·70
NOVEMBER	7·54	3·36	2·22	1·18	3·83	6·02	5·57	4·00	4·93	5·07	5·50	3·33
DECEMBER	0·83	0·76	1·06	2·67	0·91	1·62	0·86	3·23	3·26	4·53	2·64	0·93
Total	29·93	18·20	20·17	24·45	25·13	26·95	30·04	37·01	49·93	52·93	43·69	23·32

A Meteorological Table, from June to December, 1807,

By DR. CLARKE, of NOTTINGHAM.

☞ The following observations on the Thermometer are made at 8 A. M., 2 P. M., and 11 P. M.; and on the Barometer at 2 P. M. The former instrument is placed in the open air, exposed to the west, but in a situation surrounded by buildings, which prevent any alteration of temperature from currents of air. The direction of the Wind is taken from the vane of St. Peter's Church; and the numbers state how often it has been observed in any particular quarter during the month.

1807.	THERMOMETER.				BAROMETER.				WEA.		WINDS.			
	Highest.	*Lowest.*	*Mean.*	*Greatest variation in 24 hours.*	*Highest.*	*Lowest.*	*Mean.*	*Greatest variation in 24 hours.*	*Fine, or Fair.*	*Snowy, or hazy.*	*N. and N.E.*	*E. and S.E.*	*S. and S.W.*	*N. and N.W.*
JUNE, ...	75°	46°	57°·85	10°	30·31	29·53	29·95	·33	20	10	24	5	24	37
JULY.	80	52	64·00	8	30·50	29·52	29·90	·56	17	14	11	8	55	19
AUGUST, ...	78	53	64·98	9	30·18	29·59	29·85	·34	28	6	11	11	53	18
SEPTEMBER ..	67	40	51·93	10	30·15	29·21	29·69	·55	15	15	5	1	43	41
OCTOBER,....	65	40	53·29	14	30·15	29·19	29·83	·51	23	8	8	8	51	26
NOVEMBER,..	50	26	38·93	11	30·10	28·43	29·44	·80	14	16	16	2	41	31
DECEMBER,..	50	24	38·14	13	30·24	29·11	29·84	·53	25	6	2	9	48	34
Avr. for 7 Months	—	—	52°·73	—	——	——	29·78	Totl.	142	75	77	44	315	206

METEOROLOGICAL JOURNAL

For JUNE, 1808,

Kept by ROBERT BANKS, Mathematical Instrument Maker, in the STRAND, LONDON.

N. B. For want of room in the present number, the apparatus and its relative situations will be described in our next.

MAY. Day of	THERMOMETER. 11 A. M.	11 P. M.	Highest.	Lowest.	BAROMETER.	WEATHER. Night.	Day.
30	64	63	70	56	30,30	Fair	Fair
31	69	64	74	55	31,30	Rain	Ditto
JUNE.							
1	60	56	64	49	29,76	Fair	Rain
2	62	58	66	48	30,4	Ditto	Fair
3	63	60	67	57	29,79	Rain	Ditto
4	61	56	63	52	29,71	Fair	Rain
5	62	56	64	51	29,68	Ditto	Fair
6	53	52	57	49	29,72	Rain	Rain
7	58	56	62	51	29,84	Fair	Ditto
8	60	55	63	51	29.82	Rain	Fair
9	54	55	62	52	29,66	Cloudy	Rain
10	58	56	64	49	29,87	Fair	Fair
11	63	58	67	53	30,4	Ditto	Rain
12	60	59	69	52	30,18	Ditto	Fair
13	66	63	70	55	30,15	Ditto	Ditto
14	67	63	70	55	30,7	Rain	Ditto
15	64	62	68	50	29,92	Fair	Ditto
16	62	61	65	56	30,12	Ditto	Ditto
17	64	62	70	59	30,14	Cloudy	Ditto
18	67	68	74	62	30,6	Fair	Ditto
19	68	67	74	62	30,11	Ditto	Ditto
20	67	66	71	60	30,6	Ditto	Ditto
21	67	62	70	58	30,—	Cloudy	Ditto
22	69	62	72	56	29,83	Rain	Ditto
23.	62	58	68	54	29,72	Fair	Rain
24	63	62	68	55	29,89	Ditto	Fair
25	63	62	69	56	30,4.	Ditto	Rain
26	63	61	69	55	30,6	Ditto	Fair
27	61	60	62	54	30,6	Cloudy	Ditto
28	58	58	62	54	30,9	Fair	Ditto

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1808.

ARTICLE I.

Observations on the crystallized Substances included in Lavas: by G. A. Deluc.

(Concluded from p. 188.*)*

"EVERY thing in volcanoes indicates, that the depth of their foci is immense." These are the words of Mr. Fleuriau de Bellevue, and he adds, "This is the opinion of Mr. Deluc and several naturalists."

I have said, and I believe, that the foci of the volcanoes are at very great depths, contrary to the opinion of those naturalists, who imagine the foci to be very near the base of the volcano, and even place them in the cone, that rises above the ground: an opinion so repugnant to all the phenomena, that I cannot conceive how it could enter into any one's head. I do not think however, that I have used the word *immense*, which would imply a depth below the reach of conjecture, and this is far from my idea. A league perpendicular is a very great depth, and I do not suppose the foci of volcanoes can be much deeper; but every thing indicates, that they have ramifications. The fragments of natural rock they throw out can come only from these late-

Foci of volcanoes very deep,

but not of an immense depth.

Ramify among the strata.

ral galleries, from which they are broken off and carried along by the lava that flows through them. Another phenomenon indicates the same thing: this is the burning places that manifest themselves at the bottom of the sea in the environs of a volcano during an eruption, and which are at the same time a sign, that the focus is not at a depth to be called *immense*. I particularly remark this expression, because from this presumed depth have been deduced theories respecting the formation of the globe, that are destitute of foundation.

Some deeper than others.

What in fact is the depth that may be inferred from volcanic phenomena, compared with the diameter and solidity of the globe? This depth is no doubt more or less, according to the mass raised up by the volcano. Thus it is probable, that the foci of Etna, the peak of Teneriffe, and the volcanoes of Peru, are deeper than those of Vesuvius, Vulcano, and Stromboli. This is all we can conclude; and nothing respecting the origin of our globe, or the events that have concurred in its formation.

We alter the form of minerals,

"Man separates, dissolves, brings together, and combines minerals, and causes them to change their form." All this is true: he does it by his solvents, and the fire of his furnaces; but it is not added, that there is no method, no fire whatever, by which he can reftore them to their state of mineralization. He is no more capable of doing this, than of regenerating the plants he has burned and reduced to ashes. We are very far indeed from being able to produce any thing similar to the rocks, the crystals, the minerals of our mountains. This single reflection overturns every system, that ascribes the formation of these substances to fire, since all the operations of natural and artificial fire that we know, and we can reason only from these, produce nothing similar to them.

but cannot restore them to their primitive state.

We should be cautious therefore in comparing our means with those of nature.

These limits, which human means cannot pass, should render us very circumspect concerning the results ascribed to them, since no one of the natural substances, that man destroys or alters the nature of, can reappear again, but by following the laws and order established by the Creator from the origin of all things.

Limestone

Mr. Fl. de Bellevue mentions a *singular production* of a lime-

lime-kiln, which he quotes as an example in favour of his system. "This production," says he, "resembles internally certain hornblendes of the Alps, and compact and homogeneous lavas. Its external part is puffed out like that of lavas, its surface is covered with a yellow glaze, and its cavities are lined with little crystals." In a note he adds: "let not the reader suppose, that the stones of this kind had fallen accidentally into the kiln, as this was impossible."

supposed to be converted into hornblende or lava in a kiln.

I shall offer no direct objection to the fact; as this would require a knowledge of the production itself, and particularly of the vicinity of the lime-kiln: but I shall offer a general remark, that may throw some light on its origin. It is a very common circumstance, for fragments of other stones, which the workmen have overlooked, to get among the broken limestone, with which the kiln is filled, and not to be observed till the lime is taken out. In this case, instead of a piece of lime we find a stone covered on its surface with a vitreous glaze, which being broken appears to be granite, serpentine, or some other vitrifiable stone. Instances of this are frequent in the lime-kilns in my neighbourhood. To be certain, that such fragments could not be introduced, there must be nothing but calcareous rocks in the country, these must even be free from quartzose or siliceous nodules, and there must be no other kind of stone either belonging to the soil or adventitious. Thus it is very probable, and from a great number of instances I am persuaded it was the fact, that the product of the lime-kiln abovementioued was originally a stone of a different kind from that commonly burned in the kiln.

But pieces of other stone get into limekilns accidentally.

"Naturalists," continues Mr. Fl. de Bellevue, "who still believe, that rocks on which volcanic fires have acted have experienced only an imperfect fusion, and that their crystals have remained intact amid their fluid paste, are obliged to have recourse to a multitude of suppositions, to explain the state in which the lavas are found when cold."

That lavas are incompletely fluid said to be a supposition.

These naturalists have recourse to no supposition: they find it not necessary. Nothing in the lava changes its form or nature when it cools. The foreign substances it contains

But it is a fact.

in its incandescent paste retain their form: no change takes place, the fire of volcanoes not being sufficiently intense, to fuse them or alter their nature. I have adduced a great many instances of this.

Volcanic fires could not fuse solid rocks.

On this occasion I shall recal to the reader's mind the idea I suggested respecting the probable state of the subterranean strata, from which the lavas issue. We see, that to reduce stones or minerals to a state of fusion, they must be broken into very small pieces: but there are neither pestles nor stampers in the strata from which the lava originate; and volcanic fires are as incapable as those of our furnaces, to fuse rocks in a solid mass. These strata then must be in a pulverulent and muddy state, to be capable of being fused. In such a state we can easily conceive chemical affinities may exert themselves, and form crystals either solitary or in groupes, that would remain enveloped in the matter in fusion. How is this fusion effected? whence arise the fires that occasion it? We perceive from its emanations, that sulphur is the principal ingredient, that iron enters into the mixture, and that muriatic acid and sal ammoniac likewise form a part of it. But what circumstance, what combination is necessary, to excite the fermentations, that produce the fires, the fusion, and all the phenomena of volcanoes? On this we shall never be able to do more than form conjectures, some of which may approximate to the truth, and others be very wide of it. But as no means we are capable of employing can prevent any of them, it is of little importance, whether our conjectures on the origin of these fires, and the manner in which they act, be just or erroneous. All that is essential is not to ascribe to them a greater extent, more activity, and a wider influence, than they really have; that we may not be led to form systems on mistakes or exaggerations.

In these fires sulphur is the principal ingredient, with iron, muriatic acid, and ammonia.

Sea-water said not to be necessary to volcanic fires,

Mr. Fl. de Bellevue does not admit, that sea-water is absolutely necessary to produce volcanoes; and he quotes in opposition to this opinion, which at first appeared to him very plausible, a volcanic eruption mentioned by Messrs. von Humboldt and Bonpland, which took place in 1759, "in a plain in Mexico, forty leagues from the sea in a direct line; an eruption that in one night threw up a volcano of

as an eruption in Mexico 100 miles from the sea.

of 1494 feet [1592 Eng.] high, surrounded by more than two thousand mouths, which are still smoking."

If burning volcanoes could manifest themselves any where, without being within reach of the influence of the sea, we should not have a single instance of the kind quoted, for numbers would exist: and if this had been the case, I should not even have thought of the opinion I have advanced. But after having attended to this general fact, that there is no burning volcano in an inland country, and that no extent of fresh water, however large, has produced one; all being near the sea, or surrounded by its waters: and having observed, that the vapours of volcanoes deposit abundance of muriatic acid: I hence deduced this indisputable inference, that sea water is absolutely necessary, by the salts it holds in solution, to produce the fermentations that raise and feed volcanoes.

But the contrary appears from the situation & circumstances of volcanoes.

This conclusion has since been confirmed by the eruptions of water from the volcanoes in Iceland, which deposited common salt in large quantity; and lately by an observation of Messrs. von Humboldt and Buch, who were witnesses of the eruption of Vesuvius in August 1805, and perceived the sides of a cleft in its crater lined with a crust of sea salt two or three inches thick.

Sea-water thrown up by Hecla.

Clefts in Vesuvius encrusted with salt.

Hence it follows, that the fact quoted by Mr. Fl. de Bellevue proves nothing more, than that there may be subterranean channels extending forty leagues from the sea, and that on some occasion its waters penetrated into them; or perhaps their influence was merely extended gradually to that distance. It is even very probable, that, if all the circumstances accompanying this fact were fully known, a more precise explanation of it might be given. In 1538 an equally sudden eruption raised up the *Monte-nuovo* near Naples.

The Mexican volcano accounted for.

"All those who have seen volcanoes in a state of activity," says Mr. Fl. de Bellevue, "assert, that nothing is equal to the violence and immensity of their fires; and yet some appear without hesitation, to rate the power of volcanoes even below that of our paltry furnaces."

Fires of volcanoes said to be violent and immense.

A volcano during an eruption exhibits such a grand and awful sight, that it lays hold of the spectator's imagination,

But this imaginary.

and

and throws him into amazement. This is the effect of the extent of its fires, the noise with which they are accompanied, and the sight of the streams of burning lava. But if he examine it with inquisitive attention, he will soon judge from its effects, that this vast furnace has in no part a heat so intense as may be produced in our iron works; and it is easy to perceive, why our furnaces have this greater intensity of heat: it is produced by the continual current of air blown into them, which by its extreme rapidity is incessantly bringing fresh air, the presence of which imparts greater activity to the fire; but this cannot happen to the fire of a volcano, which has no such communication with the air. This is the reason why the pyroxene schoerl, which is unalterable by the fire of volcanoes, is reduced to the state of glass in a crucible in our iron furnaces, and fragments of lava exposed to a similar trial are more completely vitrified.

The heat inferior to that of our furnaces; and must be for want of a current of air.

Of all volcanic substances the obsidian, or compact glass of volcanoes, has been exposed to the greatest heat. The vitrification of this is complete. None of the pieces I possess, or have seen, exhibits any thing but glass. All the substances that compose it have been reduced to perfect fusion. These vitrifications therefore come from a part of the focus, where the heat has been urged to a higher degree by some particular circumstance.

Obsidian a perfect glass.

Now why is it that these obsidians, which must have cooled as slowly as other lavas, do not exhibit any crystalline figure within them; if it be not for this reason, that, all the substances in them having been fused, there can be nothing but glass throughout their mass?

And this includes no crystals.

I will say in my turn, that it is much more extraordinary for men to set out from the operations of our petty manufactories, to determine the force of the fires of volcanoes, and assign them an unlimited extent; and still more extraordinary thence to deduce the origin and formation of rocks and primordial mountains. Let us confine ourselves to the effects, that our narrow means can produce; and not plunge ourselves into a labyrinth of illusions, by reasoning from small to great; for, as our means are merely artificial, they are not those that operate in nature.

We must not reason from our operations to nature's.

"Naturalists,"

"Naturalists," says Mr. Fl. de Bellevue, "who imagine, that the crystals contained in lava have remained intact amid their fluid lava, pass by without notice the observation of those, who, as Spallanzani and Hubert relate, have seen the lava spout up at different times like water issuing from a fountain, form a number of very brisk streams, and possess a degree of fluidity sufficient to insinuate itself into the smallest interstices of the bodies it penetrates:" and he adds in a note: "Mr. Faujas has in his collection a piece of a palm-tree from the Isle of Bourbon, which proves, that the fluidity of the lava has been very great, since it has insinuated itself into the very fibres of the wood."

Lava said to be as fluid as water.

Piece of palm, into the fibres of which lava has penetrated.

This fact, if it were real, would prove an impossibility, namely, that lava might be in a state of fusion without being red hot: for a substance as combustible as a piece of a palm-tree, or any other vegetable, would have been burned and consumed, or reduced to a coal at the first contact of the lava. It must be an illusion therefore; and either the substance surrounding the palm is not lava, or the matter surrounded is not a vegetable substance. This illusion, strange as it is, is not single in its kind. In an account of a tour to Iceland, translated and published at Paris in 1802, I have read, that the Danish travellers imagined they discerned wood in a piece of lava of Hecla. Count Borch made the same mistake, and even greater, for he says he saw "pieces of wood slightly scorched" in whole rocks of the lava of Etna.

This an impossibility.

Others have made the same mistake.

I have in my possession a large piece of vitreous lava, that I brought from the island of Vulcano, which may serve to explain this illusion. It has very large blebs, which are drawn out considerably in length by the flowing of the lava, and their surface is streaked with threads, which have the appearance of woody fibres; and this appearance is heightened by the tint they have derived from the vapours, that are continually exhaling from the matter in fusion. Several persons, who have seen this fragment, have taken it at first view for wood. I have another piece of vitreous matter from Lipari, which is drawn out into such fine and close threads, that no fossile agatized wood, let its fibres be ever

Vitreous lavas sometimes closely resemble fossil wood.

so distinct, would have a stronger resemblance of wood than this, were it not for its glassy lustre. I have another piece, vitreous likewise, one of the surfaces of which, that was exterior, is marked with a multitude of very small threads, arranged in some places in undulations resembling the woody fibres round a knot.

This the case with the supposed piece of palm.

From these examples I am led to believe, that the specimen from the Isle of Bourbon is wholly lava, with a woody appearance on one of its faces; for at all events a vegetable, even in the state of wood, could leave nothing after its combustion, which must be inevitable, but a vacuity in the lava, and traces of charcoal; and by no means the impression of woody fibres, still less the fibres themselves.

Grand eruptions flow like water,

In support of his principal opinion Mr. Fl. de Bellevue adduces several arguments, which I shall pass over, because our business is with facts, not conjectures. One of them is the following. "The great volumes of lava, that act the principal part in volcanic eruptions, burst out from the crater, from the sides of the mountain, or from its base. They proceed with rapidity from the very focus of the volcano, possessing an incomparably greater degree of heat than the matter that rests in the crater. This heat, this rapidity, cause them to spout forth and flow like water, and cannot permit any crystals to form in them. All that are found in it afterward are produced during its repose and refrigeration."

therefore the crystals formed in it while cooling.

Flowing like water a metaphorical expression.

I must first remark, that these expressions frequently repeated, that lava spouts out and flows like water, are merely metaphorical; for lava, far from flowing like water under any circumstances, leaves in succession, by hardening, all its matter on the ground it flows over.

Eruption of Etna of 1669.

Mr. Fl. de Bellevue did not recollect the lava of Etna of 1669, which I have already mentioned. This lava, issuing from the base of that great volcano, traversed a space of ten miles in length, and advanced into the sea, where it accumulated in prodigious heaps, after having covered its route with its matters to a vast extent in breadth and thickness. This certainly was to be reckoned among the lavas, that act the principal part in volcanic eruptions. Now this lava, of which I have pieces before my eyes, I again assert

is

is filled throughout its whole extent, from the extremity of its destructive course to the place where it issued from the crater, with a multitude of pyroxene schoerls, of those whitish crystalline laminæ I have described, and of small chrysolites; and the crater, from which it issued, threw up myriads of the same substances. Can we discern here formations produced at the time of the cooling of this lava, since all these crystals existed there at the moment of its greatest fusion and heat, the focus of the eruption itself having thrown up loose ones from its crater in multitudes innumerable?

Leucites and pyroxenes not found in the strata we have penetrated, but come from others beneath.

The naturalists who have remarked, that leucites and pyroxene schoerls are crystals not to be found in the strata coming under our observation; and who have hence inferred, that they would have remained for ever unknown to us, if volcanic eruptions had not brought them to light; are certainly well founded in their opinion. Mr. Fl. de Bellevue however terms it a supposition. But nothing is more true than the observation, and nothing can be more natural than the consequences deduced from it.

Rocks of the primitive kind not now formed in water.

"We have seen," continues he, "that there is no example to prove, that aqueous solutions now form, or are capable of forming, rocks similar to the primitive ones; and that fire on the contrary daily exhibits to us productions, that are not simply analogous, but even identical with them."

On the contrary we have seen, that the productions of fire have only an apparent, not a real resemblance to primitive, or, to speak with more accuracy, primordial rocks. The fires of volcanoes, like those of our furnaces, have not produced and never will produce any thing like them, because these primordial strata do not owe their origin to fire.

because their principles, which existed in the primitive ocean, are no longer found in the sea.

Neither will aqueous solutions form such rocks; for they were produced by precipitation from the primordial fluid at periods not remote from the origin of the globe, and every thing indicates, that they no longer continue to be formed. The water of the present sea does not now contain the requisite elements, for of these it has been deprived. The mud of rivers, of which some imagine they may

The deposi-

tions of rivers may be formed, but which cannot form them, does not reach the bottom of the sea: its waves drive it back, and keep it on the shore, where it adds daily to the first boundaries of the continents.

cannot form them,

and are too small to affect the level of the ocean.

On this occasion I shall repeat a remark I have several times made. These additions are so trifling, compared with the extent of the sea, that they cannot produce any perceptible change in its level. It is these additions to the land that have been so often mistaken, and quoted as proofs of the retiring of the sea.

Volcanoes could not be distinguished from other mountains, if they did not differ from them.

By what signs can we know ancient volcanoes wherever they exist remote from the sea, if not by their form, and the nature of the substances that distinguish them? They must then be different from all other mountains, or they would be confounded together, and these could not be distinguished from volcanoes. The truth is then, that all the mountains we know, the Alps, the Jura, the Pyrenees, and all those of our continents, have no relation to volcanic mountains; that their strata, and the matters that compose them, have been formed in water, and fire has had no concern in their production.

Valley of Quito

It was from these distinguishing and invariable characters of volcanoes, and of the soil around them, that in my preceding observations I employed the following expressions. "When the valley of Quito, and the mountains that border it, shall be observed by naturalists experienced in the knowledge of volcanoes and volcanic substances, I have no doubt they will perceive, that the state of things is as I have said." I should have been far from thus expressing myself, if other lands and other mountains had been the subject. But the great mountains that skirt that celebrated valley on either hand being certainly volcanoes, three of which are not yet extinct, and its soil being composed of their vast eruptions, I could venture to give this opinion without apprehension of going too far, or of wanting that proper diffidence a man ought to have in his own knowledge.

and the volcanoes bordering it.

Ancient volcanoes fewer than some assert.

The ancient volcanoes observed on the surfaces of continents are not so numerous as Mr. Fl. de Bellevue imagines, when he says, that volcanoes, either burning or extinct, are seen every where on the face of the globe. This is a great

great exaggeration. Many are seen in various places, no doubt; but the space they occupy bears no comparison with that where there are none. I include ancient and extinct volcanoes, for those still burning are very few. There are only four in Europe: those of Iceland are in a distant latitude.

Burning ones very rare.

This reminds me of a similar opinion of Mr. Patrin, which he gave of Italy. It is in his *Recherches sur les Volcans*, "Inquiry concerning Volcanoes according to the Principles of the pneumatic Chemistry." He says, "Italy is full of volcanoes, and covered from one end to the other with lavas and tufas of enormous thickness." Yet the true fact with respect to Italy is, that the Apennines, which traverse it from one extremity to the other, all the ramifications of that chain, and all the eastern shore of that peninsula, have nothing volcanic in them; and that the soil of this kind lies only on the western coast, where it is frequently interrupted by aqueous strata.

Patrin's misrepresentation of Italy in this respect.

When explanations of the manner, in which a fact in terrestrial physics, that is in some degree obscure, may have happened, deviate from the most natural, and that which is most conformable to all the phenomena, they may be very different from each other, or even opposite. Thus it happens, that the naturalist I have just quoted, being equally of opinion, that the pyroxene schoerls did not preexist in the lava, separates them from the matter of the lava, and makes them arise "from an aeriform fluid, which has passed to a solid consistence by the effect of attraction." This question I have already discussed with precision, and to some extent, founding all I have said on facts, in my Observations on Pyroxenes, or volcanic Schoerls, in the *Journal de Physique* for March, 1801.

When people deviate from nature they often support the same opinion by contradictory arguments.

From all the facts I have adduced the following conclusions may be considered as established.

General conclusions.

That every volcano, whether burning, extinct, or ancient, whatever its height or extent, and wherever situate, is a mountain of a class distinct from all others: that it is formed by no *neptunian* strata: that all the solid substances constituting it are the products of fire: that it has been raised up, from its base to its summit, by the accumulation of

Volcanic mountains formed by fire, and different from all others.

of matters successively thrown up by its eruptions, the focus of which is beneath all the strata with which we are acquainted.

Crystals in lava foreign to it.

That the crystalline substances included in lava are foreign to it: that they have been formed anteriorly in the humid way in strata, which the volcanic fires have reduced to fusion leaving their crystals untouched, because those fires had not sufficient intensity to fuse them.

The whole mountain is a volcano.

That we should cease to say volcanoes manifest themselves on the summits of mountains, because volcanic mountains entire constitute volcanoes. This is the reason why new mouths frequently open in their sides, or at their base.

Sea water necessary to it.

That sea water is absolutely necessary, by the salts it holds in solution, to excite the fermentations that produce volcanoes.

All other mountains and hills produced by water.

That all the strata and substances, which compose calcareous, schistous, or granitic mountains, and all their varieties, as well as sandy, gypseous, and argillaceous hills, are the work of water.

All ancient volcanoes have burned under the sea.

That all the ancient volcanoes, which are now inland, have burned underneath the waters of the sea. The schists and granites which appear around some of them are foreign to them, belonging to strata through which the eruption forced a passage, and which have remained bare. They would have been buried under the volcanic matter, to be seen no more, if those volcanoes had been longer active. Those which were burning at the time when the sea retired from our continents ceased to burn at that period: a period beyond the memory of the inhabitants of the country, because there could be no inhabitants of the land round those volcanoes, when it formed part of the bottom of the sea.

Volcanic sand between two calcareous strata.

Among the numerous facts that prove this truth, count Marzari of Vicenza has furnished me with a very remarkable one, on his return from a tour in Auvergne. At Santourgue there is a stratum of volcanic sand, about six inches thick, between two calcareous strata. After a calcareous deposition had been formed therefore on the sides or base of the volcano, an eruption must have thrown out and spread

spread this sand, upon which a fresh calcareous deposition took place; and these operations could have occurred only in the sea. Count Marzari has had the goodness to present me a specimen of this sand, which is similar to what was thrown out of the superior aperture of Etna in the eruption of 1763, which I have mentioned above.

I shall remind the reader here of what I have said several times, that to distinguish the different periods when volcanoes have been burning, and not to confound them together, it is proper, to call those *ancient*, which have burned in the sea before our continents were laid dry; and those only *extinct*, which by their situation are still capable of burning, if the inflammable matters that gave rise to them were not consumed. But this necessary distinction will never be made, as long as it is believed, in spite of the dictates of observation and experience, that burning volcanoes may exist independent of the waters of the sea. Distinction between ancient and extinct volcanoes.

Mr. Fl. de Bellevue must be convinced, that my sole object, in making these observations, is to illustrate in a more precise manner the grand phenomenon of volcanoes, that we may not ascribe to them effects in which they have no concern, or deny them those they have really produced. These limits, grounded on well established facts, can alone free us from systems founded on contrary notions, and afford more certain bases to geology, that interesting and important branch of terrestrial physics. The author's object.

II.

Experiments on Molybdena. By Christian Frederic Bucholz.

(Concluded from p. 196.)

(Concluded from p. 196.)

VIII. *Manner in which molybdena comports itself with certain acids.*

Exp. 29. TEN grains of powdered molybdena were put into half a drachm of sulphuric acid of the specific gravity of With sulphuric acid.

of 1·96, and left for twenty-four hours at an ordinary temperature. The acid did not exert the slightest action on the metal. At a moderate heat a large quantity of sulphurous acid was evolved, the liquor became of a yellowish brown colour, and it assumed a sirupy consistence. It was then diluted with four times as much water, and became of a brownish yellow. After standing some time a little molybdena was deposited, which had not been dissolved. The liquor having remained some hours in contact with the metal, it gradually turned green, and afterward blue: but the most remarkable circumstance was, that part of the blue oxide precipitated itself in the form of a very fine powder. The cause of this phenomenon deserves inquiry.

Converted into a yellow oxide.

This experiment teaches us, that the molybdena had been changed by the action of sulphuric acid into a yellow oxide, containing more oxigen than the green and the blue, which passed to the state of green oxide in consequence of a disoxidation produced by the contact of metallic molybdena.

With nitric acid.

Exp. 30. In treating on the oxigenation of molybdena, I have already had occasion to say something of the action of nitric acid on this metal. The experiments I shall relate will serve as a continuation. Ten grains of powdered molybdena were put into two drachms of nitric acid diluted with an equal quantity of water. At the expiration of a quarter of an hour there was a slight evolution of nitrous gas, and a pale red solution was formed. To accelerate the action of the acid I employed a gentle heat, when the molybdena soon disappeared, and the liquor assumed a yellowish brown colour with a tinge of red. I added ten grains of molybdena two different times; and when I had added the last ten grains, the liquor, which had been clear, grew turbid, and became of a carnation red. This, added to a slight evolution of nitrous gas, led me to conclude, that the acid was completely saturated. After standing a little while, blue oxide was perceived to form at the bottom of the vessel, where a little molybdena still remained undissolved: a phenomenon similar to that observed in the solution by sulphuric acid. Twenty-four hours after, the matter that rendered the liquor turbid was separated, and it comported itself in all respects like molybdic acid.

A solu-

A solution of molybdena by nitric acid, made without heat, became perfectly clear in a few hours, and of a yellowish brown inclining to red. It had a slightly acid taste, that left behind it a bitterness with somewhat metalline. Part having evaporated by a gentle heat in a porcelain capsule, it left a pulverulent residuum of a dirty reddish yellow, which, being put into a small quantity of water and shaken, was entirely dissolved, except a small portion, that was molybdic acid. The solution was yellow inclining to red: and on being digested with metallic molybdena it became blue. Solution in nitric acid without heat.

Twenty grains of powdered molybdena having been put into a drachm of fuming nitric acid, an extremely vivid effervescence took place, attended with an extrication of red vapours, and the mixture became consolidated into a mass of a light brownish red colour. Another drachm of the same acid being poured on this, and moderately heated, white molybdic acid was very readily produced.

Exp. 31. The reddish solutions obtained in the preceding experiments being filtered, ammonia, cautiously added, produced a flocculent precipitate of a brownish red colour, which, having been washed and dried, yielded a powder of a lighter hue interspersed with white and shining crystalline particles. Some of this powder was put into a small quantity of water, at a moderate temperature, and shaken, in which it all dissolved except a few small white crystals. These crystals however were not molybdic acid, for they were much more soluble, and had a much stronger acerb taste. The solution of the brown powder was of a vinous yellow colour inclining to red. The water, with which this powder was washed after its precipitation, had a deeper colour, because the precipitate was more soluble after being wetted. On adding ammonia, or potash, to the solution, a brown red precipitate fell down slowly once more. This precipitate being treated with a solution of alkaline carbonate, it was not attacked, but the white crystals were dissolved with effervescence. Farther experiments are necessary, to determine the nature of the products formed in this process. I shall here confine myself to the remark, that the brown precipitate cannot be taken for the brown oxide The nitric solutions examined.

oxide obtained by the decomposition of molybdate of ammonia, for this oxide appears to be insoluble in water: and besides, the precipitate does not furnish blue oxide with molybdic acid, but only with molybdena in the metallic state, which indicates a higher degree of oxigenation than that of the blue oxide.

With muriatic acid.

Exp. 32. Ten grains of powdered molybdena were put into a drachm of muriatic acid of the specific gravity of 1·135, and left for twenty-four hours. The acid exerted no action on the metal, it remained in the same state: and even after it had been boiled to dryness, a second drachm of acid added, and this boiled on it a few minutes, no effect was produced.

This fact appearing to me inconsistent with the property I had observed in metallic molybdena of being converted into blue oxide after having been simply wetted, I tried muriatic acid diluted with water. The metal however was not attacked, whether I employed one, two, or three parts of water to one of acid, and digested the metal in it for a long time, or boiled it.

Wetted molybdena not oxided by the water, but by the air.

Thus it appears, that, when powdered molybdena is simply wetted, the oxidation is not produced by the water, but by the oxigen of the atmosphere; the water serving only to conduct the oxigen, and dissolve the oxide formed, so that the metal continually presents a fresh surface to the action of the air.

Oxigenized muriatic acid.

Exp. 33. Ten grains of metallic molybdena were put into three ounces of water saturated with vapour of oxigenized muriatic acid: the mixture was shaken a little, and a blue solution void of smell was produced. But the greater part of the metal was not dissolved: nor was it by the addition of six ounces of acid. The liquor when filtered was of a fine blue colour; but on adding liquid oxigenized muriatic acid, the solution became as clear as water; and when more molybdena in the metallic state was put into it, the blue colour reappeared.

Arsenic acid.

Exp. 34. Ten grains of molybdena were put into a drachm of liquid arsenic acid containing half its weight of dry acid, and left to stand twenty-four hours in a closely stopped bottle. At the expiration of this time a thin stratum

tum of the liquor, about half a line thick, was of a brown yellow colour. Having boiled the mixture and evaporated to dryness, diluted the residuum in half an ounce of water, and shaken it slightly, I had a fine blue solution, and but little of the metal appeared to be left unaltered. Thus the metal had here been oxided at the expense of the arsenic acid, and converted into blue oxide.

Exp. 35. Ten grains of molybdena, half a drachm of phosphoric acid, and a drachm of water, were put into a phial, which was stopped close, and left to stand twenty-four hours. No effect was produced, and the mixture was boiled to dryness. When the residuum was nearly dry, a vapour exhaled, which had a little of the smell of phosphorus, accompanied with something like that of an alkaline lixivium when boiling down. The flame of lighted paper held over it assumed a greenish yellow colour. The residuum was heated red hot, but no stronger smell was given out, that could lead me to suppose the phosphoric acid had acted on the molybdena; and in fact when the mass was cooled and diffused in half an ounce of water, the greater part of the metal remained at the bottom, without having undergone any alteration. The supernatant liquor was of a yellowish brown colour, had a strongly acid taste, and left a metallic taste on the palate. A similar quantity of water was evaporated from the metal several times, but I did not observe the least change, and no blue oxide was formed. A small quantity of this solution was evaporated to dryness, and a grayish blue matter remained, which, on dissolving it again, to my great surprise assumed a yellowish brown colour. Ammonia added to the solution gave it a dull colour, without producing any precipitate: it was not till after the expiration of four and twenty hours, that a few brown flocks separated. Phosphoric acid.

Exp. 36. Having treated molybdena in the same manner with boracic acid, at the end of a few hours the liquor assumed a slight blueish tint, which did not increase afterward, even when evaporated and the residuum again dissolved. Thus it appears, that boracic acid has no action on molybdena, and was not the cause of the slight blue colour observed. Boracic acid.

 I had

Succinic, tartarous, citric, and acetic acids.

I had similar results with the succinic, tartarous, and citric acids: only I observed, that, in treating molybdena with succinic acid, the liquor became green during evaporation. Acetic acid produced no effect on it cold; but when boiled, and the liquor reduced to about half, it assumed a brownish yellow colour. Ammonia scarcely rendered the solution turbid.

General conclusions on the action of acids.

From what has been said it appears:

1st. That, whenever molybdena is dissolved by acids, it becomes oxided at their expense, and consequently can be dissolved only by those acids, which, like the nitric, sulphuric, oxigenized muriatic, phosphoric, and arsenic, are susceptible of several degrees of oxidation, and capable of parting with oxigen, either at the common or a higher temperature.

2dly. That by the action of acids molybdena may be brought to the state of blue oxide, and sometimes of brown, the nature of which is yet to be examined. The phosphoric alone appears to produce a different state.

3dly. That these solutions can scarcely be considered as salts, on account of the acid nature of the oxide of molybdena.

IX. *Action of potash on the native sulphuret of molybdena.*

Potash with the native sulphuret of molybdena.

Exp. 37. On fifty grains of pure sulphuret of molybdena I poured a lixivium containing two hundred grains of pure caustic alkali, evaporated to dryness, diluted the residuum in water, and evaporated again. After repeating this several times, I separated the undissolved part by filtration, washed, and dried it. The loss amounted to scarcely four grains, and what remained had the same appearance as before. On this I poured sulphuric acid diluted in water, but no sulphuretted hidrogen gas was evolved. The filtered liquor has a strong taste of sulphurous acid; diluted sulphuric acid expelled from this a large quantity of sulphuretted hidrogen gas; its colour, which was a pale brownish yellow, changed to brownish red; and at the end of a few minutes a fine brownish red precipitate was formed, which gradually changed brown, and thence to a yellowish brown; the liquor becoming a pale reddish brown. The precipitate

precipitate when dried was of a chocolate colour, and weighed 3½ grains. This appeared to be a simple hidrothianat [hidrosulphuret] of molybdena: for when heated with muriatic acid a small quantity of sulphuretted hidrogen gas is evolved, and when heated redhot in a crucible it does not give out the blue flame of sulphur, but simply a smell of sulphurous acid. On decomposing it with nitric acid, it immediately gave out sulphuric acid, which was rendered evident by barytes.

Exp. 38. Twenty-five grains of sulphuret of molybdena were put into a lixivium containing a hundred grains of caustic alkali, evaporated, and heated redhot for a quarter of an hour. As soon as the alkaline mass began to flow, the alkali acted so powerfully on the molybdena, that the whole of the metal seemed to be fused by it. The mass had assumed a cherry red, which soon passed to a deep crimson. The water in which this was diffused acquired a deep green colour, which it lost in a few hours by mere exposure to the air, and became of a blackish gray. The residuum, after being washed and dried, was of a light gray colour, and weighed twenty grains. Its nature will soon appear.

Heated redhot and fused. Part dissolved. Residuum.

Sulphuric acid and muriatic acid diluted with water, and added in excess to the solution that had passed the filter, extricated from it sulphuretted hidrogen gas, and occasioned a precipitate similar to that of the preceding experiment. A part of the molybdena formed with the free acid a blue solution above the precipitate. Nitric acid occasioned a similar precipitate: but the blue liquid, that contained it, became greenish, and afterward of a reddish yellow, in consequence of the progressive oxidation.

Solution examined.

The experiments related indicate, that the alkali (potash) exerts but little action on molybdena in the dry way, and still less in the wet. I thought, that, if the quantity of sulphur were increased, the action might be more considerable, and accordingly I made the following experiment.

Potash acts but little on it.

Exp. 39. I took ten grains of powdered molybdena, which I put into half an ounce of an alkaline lixivium holding in solution twenty grains of sulphur. This I boiled and evaporated almost to dryness twice. The matter, as in the 38th experiment, was of a cherry red round the edge.

Treated with potash and sulphur.

On diffusing it in water the solution was of a fine deep green. The molybdena however did not appear to be attacked in any sensible degree. Forty grains of sulphur were added, and the process as above was repeated three times. The molybdena was still found to be but little altered, and had lost only two grains. The solution being decomposed by sulphuric acid, it yielded only a grayish blue precipitate, the aspect of which was perfectly like what is called *lac sulphuris*, and contained a few flocks of a yellowish gray.

In the dry way. *Exp.* 40. I then took two drachms of alkaline lixivium, thirty grains of sulphur, and ten grains of molybdena; put the whole into a Hessian crucible; evaporated to dryneſs; and left it in a red heat for a quarter of an hour. The mass being diffused in eight ounces of water, and filtered, the undissolved residuum weighed three grains. The solution was of a fine yellowish red, and sulphuric acid produced in it a blackish brown precipitate, which was in no respect altered by an excess of the same acid: the liquor gave no sign of a blue appearance: and the precipitate, after being separated, washed, and dried, was of a brownish black, and weighed forty-five grains.

Precipitate examined. This precipitate was not altered by boiling in sulphuric acid, and afterward with muriatic: but when nitric acid was added to the muriatic, and it was boiled again, it was decomposed and dissolved, with the exception of a little sulphur. A solution of barytes indicated the presence of sulphuric acid. Five grains of the precipitate, having been heated redhot in a small glass, gave out about two grains of sulphur. The residuum was speedily oxided by nitric acid, but still a little sulphuric acid was found in the solution, which proves, that the action of the fire had not separated all the sulphur. From what has been said it follows, that the precipitate was composed of molybdena in the metallic state, or approaching to it, of hidrothianat of sulphur, and of a slight excess of sulphur; while the precipitates in the 36th experiment were composed of oxide of molybdena combined solely with sulphuretted hidrogen, or at most with a little sulphur. This experiment having been repeated with four times the quantity of molybdena, and the

the roasting continued a quarter of an hour longer, gave the same results.

X. *Action of hidrothianates of alkaline sulphurets, and of pure hidrothian acid, on molybdic acid.*

Exp. 41. I dissolved molybdate of ammonia in twenty times its weight of water, and added sulphuric acid, till the precipitate formed was entirely redissolved. I then poured in hidrothianate of ammoniacal sulphuret, and a reddish brown precipitate was formed, which was more or less considerable, and the supernatant liquor was more or less blue, according as the quantity of sulphuric acid and of water employed to dissolve it was greater or less. I found too, that on adding a small quantity of hidrothianate of ammoniacal sulphuret to the solution of molybdate of ammonia, the sulphuric acid produced no precipitate, but merely rendered the solution blue; while a precipitate took place, if there were a larger quantity of hidrothianate of ammoniacal sulphuret: thus in one case all the hidrothianate of sulphur is employed in disoxiding the molybdic acid. Solution of molybdena in sulphuric acid treated with hidroguretted sulphuret of ammonia.

Exp. 42. Five grains of sublimed molybdic acid dissolved in ten drops of concentrated sulphuric acid were put into five ounces of water. Hidrothianate of sulphuret of ammonia occasioned in this a chocolate coloured precipitate, which was almost black when dried. An excess of acid did not decompose it, or produce a blue colour: thus it was similar to the native sulphuret of molybdena. Molybdic acid.

Exp. 43. Molybdate of ammonia was dissolved in twelve times its weight of water, sulphuric acid added in excess, and solution of sulphuret of potash poured in. This occasioned a light reddish brown precipitate, and the liquor became blue. Sulphuric acid being added to a solution of molybdate of ammonia merely to saturation, hidrothianate of sulphuret of potash occasioned a flesh coloured red precipitate inclining to a copper colour. In a solution to which no sulphuric acid had been added, no precipitate was occasioned by the hidrothianate, the liquid merely becoming a little milky, which might be expected from the property I have already observed the sulphuret of potash possesses of dissolving molybdena. The acid added afterward produced anew Molybdate of ammonia with sulphuret and hidroguretted sulphuret of potash.

anew a precipitate of a reddish brown colour. All these precipitates were decomposed by an excess of acid, a blue solution was formed, and nothing remained at bottom but sulphur of a brownish gray colour containing a little molybdena.

Sulphuretted hidrogen gas passed into a solution of molybdic acid.

Exp. 44. Two phials were connected together as in Woulfe's apparatus. In one, which served as a receiver, there was a solution of one drachm of molybdic acid in eight ounces of water: in the other there was an ounce of sulphuret of lime with eight ounces of water, and sulphuretted hidrogen gas was evolved. As soon as the gas began to pass through the solution, this assumed a reddish brown colour, which became deeper and deeper, but still continued clear. I took a little, which smelled strongly of sulphuretted hidrogen, added to it some muriatic acid, and a blackish precipitate was formed. At the expiration of four and twenty hours the whole of the solution became a little turbid; and after exposure to the air for twelve hours in shallow vessels it was completely turbid, opake, and of the colour of mud. Heated afterward to ebullition, it resumed its clearness and colour, except that it was a little more inclining to yellow. The froth that formed during the boiling was of a fine reddish yellow, like tincture of saffron. While it was evaporating to dryness by a moderate heat, a smell of sulphuretted hidrogen continued to be given out, and toward the end a great deal of ammonia was evolved.

Brown residuum examined.

The residuum, weighing fifty-five grains, was of a light brown chocolate colour, and exhibited the following properties. 1. Ten grains being exposed to a moderate heat, a pretty large quantity of ammonia was evolved, accompanied with a smell of sulphuretted hidrogen. This smell alone was perceived when the heat was increased: at length sulphurous acid was given out, and the matter assumed a blueish black colour. It now weighed eight grains, was insoluble in water, and in the acid a little concentrated by a mean temperature. Thrown into a redhot crucible, it immediately became red, sulphurous acid vapours were expelled, and it melted. This was molybdic acid. 2. Ten grains of the residuum put into a drachm of muriatic acid, and heated to ebullition, gave out but little sulphuretted hidrogen; and formed

formed a brownish yellow solution, which on dilution with water assumed at first a blueish green colour, and afterward became completely green. A similar quantity having been previously shaken in water, and afterward put into muriatic acid, gave rise to a pretty considerable evolution of sulphuretted hidrogen, and produced a blue solution, which soon assumed a tinge of green, and let fall a blue precipitate insoluble in water. This I had an opportunity of observing in several experiments. Its external appearance greatly resembles that of the blue oxide of molybdena, from which it differs however, since it is not soluble like it in water. It requires farther examination therefore, to determine its nature. 3. Five grains of the dried residuum were put into half an ounce of cold water, and shaken; but no effect was produced. Being boiled for a quarter of an hour, part was dissolved, leaving two grains of a fine reddish yellow colour. The solution had the same colour as the preceding: it emitted a strong smell of sulphuretted hidrogen: the sulphuric acid increased this smell, and changed the solution at first blue, afterward green.

A compound of sulphuretted hidrogen, ammonia, and molybdena.

From all these circumstances it appears, that the residuum is a triple compound of hidrothian acid, ammonia, and molybdena. With respect to the acids it comports itself like the precipitates obtained in the experiments 38, 41, and 43. After being roasted to redness it approaches the native sulphuret of molybdena, from which however it appears to differ still by retaining a small portion of sulphuretted hidrogen. It is much more quickly converted into acid by the action of fire than the sulphuret of molybdena.

Sulphuretted hidrogen gas passed into water in which undissolved molybdic acid was diffused.

Exp. 45. Ten grains of very pure molybdic acid, first fused, then powdered, and afterward boiled in ten ounces of water, which dissolved but a very small part, were put into the same apparatus as that of the preceding experiment, and subjected to the same treatment. As soon as the sulphuretted hidrogen gas began to pass over, the liquid became brown: the colour grew deeper and deeper, and the greater part of the molybdic acid, which swam in the solution, was dissolved: nothing remained at the bottom but some brownish black flocks. At the conclusion the liquor assumed the same colour as in the preceding experiments, and

and had a strong smell and taste of sulphuretted hidrogen. At the end of four and twenty hours it became turbid, and deposited a pretty considerable quantity of a yellowish brown powder, which was separated and dried, when it became of a brownish black. The filtered liquor was of a yellowish brown: when made to boil, sulphuretted hidrogen gas was evolved; a larger quantity of powder was precipitated; and it retained but a slight smell of sulphuretted hidrogen, which the addition of a few drops of muriatic acid rendered stronger, at the same time producing a blue colour. The precipitated powder, put into muriatic acid and exposed to a moderate heat, comported itself like the residuum of the preceding experiment; but boiling it ultimately produced a solution of a brownish yellow colour. A little of this powder, being thrown into a redhot crucible, burned immediately with a sulphurous flame, which soon disappeared.

Pure molybdic acid will combine with sulphuretted hidrogen.

This experiment shows, that pure molybdic acid is likewise capable of combining with hidrothian acid; but this combination is not as constant as that of the preceding experiment, in which ammonia too is present. It proves the variations, that the less limited disoxigenizing action of the hidrothian acid must produce. Thus by dessication simply it passes to the same state, to which the compound of the preceding experiment is not to be brought but by a much stronger heat; and by the oxidation of a part of the hidrogen it forms a hidrothianate of sulphuret of molybdena, that gives out in roasting a vivid sulphurous flame, which the native sulphuret of molybdena does not, and is converted into molybdic acid.

It remains for me yet to examine the action of hidrothian acid on molybdena in the same respects as in the 41st experiment.

Molybdate of ammonia decomposed and redissolved by sulphuric acid, and treated with sulphuretted hidrogen.

Exp. 46. Sulphuretted hidrogen was passed in the manner already mentioned through a solution of a drachm of molybdate of ammonia in four ounces of water, which had been decomposed and redissolved by three drachms of rectified sulphuric acid. In two or three minutes the solution, which was before like water, assumed a blue colour. Five minutes after a light chocolate brown matter was deposited

on

on its surface, and on the wet sides of the vessel; but this disappeared after a few minutes. The fine blue colour of the solution changed to a black, and a precipitate of the same colour fell down. The liquor having been filtered, and set on the fire, became again of a fine blue by boiling. The water with which the precipitate was several times washed was also blue, but the colour had little intensity. The precipitate, having been dried, was of a blueish black, and exhibited the following results. 1. Boiled in moderately concentrated muriatic acid, it yielded a brownish yellow solution. 2. Thrown into a crucible at a dull red heat, it burned with a fine blue flame; which in a crucible at a bright red heat was quickly over, but there was a very considerable extrication of sulphurous acid after the flame had ceased. The residuum left after the combustion with flame was of a blackish brown, insoluble in water, and reducible to molybdic acid by increasing the heat. Put into water and shaken it gave a light blue tinge after some time. The residuum separated by the filter had lost its brown hue, and appeared almost entirely black. These experiments indicate, that the molybdic acid had been at first disoxigenized, and that afterward it entered into combination in the brownish black precipitate, which appeared to contain a little blue oxide; a circumstance that seems peculiar in this case, and merits investigation; but which in other respects comported itself as in experiment 45.

General remarks on the action of potash,

From all the experiments repeated under the 9th and 10th heads it follows: 1st. That potash exerts scarcely any action on sulphuret of molybdena in the humid way; that this action is more considerable in the dry way; and that in dissolving afterward in water a greater or less combination of sulphuretted hidrogen with sulphur takes place.

sulphuret of potash,

2dly. That the sulphuret of potash comports itself in the same manner. From compounds formed in the dry way acids precipitate a matter, which is a sulphuret of molybdena containing a small portion of sulphuretted hidrogen, and which comports itself with acids nearly as native sulphuret of molybdena.

hidrogurretted alkaline sulphurets,

3dly. The hidrogurretted alkaline sulphurets precipitate from the solution of molybdic acid a matter of a colour similar

milar to that of chocolate, which imparts a blue colour to the acids in which it is dissolved, and appears to differ from the preceding in the oxidation of the molybdena, and in containing more sulphuretted hidrogen and less sulphur. Thus we have two compounds of this kind; and the latter appears capable, under certain circumstances, of being converted into the former.

and sulphuretted hidrogen.

4thly. Pure sulphuretted hidrogen gas equally combines with molybdena, exhibiting phenomena that indicate a disoxigenation, and forms products similar to those resulting from their combinations. The passage of this gas through a solution of molybdate of ammonia gives rise to a triple compound, which is soluble in water, decompofable by heat, and rendered by it similar to the native sulphuret of molybdena.

Conclusion.

I here conclude the publication of my experiments on molybdena. I am free to confess, that they do not exhibit a complete work; but I flatter myfelf, that some conclusions may be drawn from my labours not altogether unimportant to the science of chemistry. Besides it was necessary, that such experiments should be some time made; and I can aver, that I employed all the care and attention possible, so that complete reliance may be placed on their accuracy. Farther experiments will complete what I have begun.

Farther experiments promised.

These I shall undertake, as soon as I have procured a sufficient quantity of molybdena, and my occupations will afford me leisure.

III.

On the native Gold Dust found in the Hills in the Environs of the Commune of St. George's, in the Department of the Doire: by Mr. Giulio, *Prefect of the Department of the Sesia**.

Native gold found in the sands of rivers.

It has long been known, that a great number of rivers and brooks carry with them particles of native gold, of larger

* Journal des Mines, No. 116, p. 145.

larger or smaller size: that independently of the places where this metal is found in its native situation, it is disseminated in grains in their sands, as those of the Rhone, the Arriège, and the Cèze in France, and with us in those of the rivers Doire, Balthée, Cervo, Elbo, Mallon, and Orba, and of the rivulets Oropa, Orémo, Evançon, Vison, &c. It is equally known, that there are persons, who make it their whole business, to search for this gold.

Mineralogists are not perfectly agreed respecting the origin of this gold dust: for the oldest, and among the moderns Brochant, suppose this gold was originally brought from mines, commonly situate in primitive mountains, from which it has been washed down by the water of the rivers. "Native gold," says Brochant*, "is found chiefly in primitive mountains, where it is met with in veins, and sometimes disseminated in the rock····it occurs also in alluvial strata, where it is frequently wrought with advantage. The sand of several rivers is mixed with grains of gold, which are separated from it by washing. It is unquestionably evident, that the gold here is met with accidentally; and that it is deposited by the water, that has washed it away from its original situation, which was probably the same as is indicated above." Its origin disputed. Brochant.

Others on the contrary think, that these metallic particles were originally disseminated in auriferous strata, in the very places where they are exposed to view by great floods, or overflowings of the rivers, or that they have been washed into the latter by torrents in storms or heavy rains. Supposed to be in their original situation.

I do not intend to enter into the question generally, or at large. This I leave to the learned, whose chief study is the improvement of the science of mineralogy. My inductions go no farther than the small number of researches I have made: yet I think I may venture to say, from the observations I am about to present the reader respecting the locality and situation of the native gold dust in the commune of St. George's, that such dust is not always washed down from mines in the mountains by rivers. And if such At least not always washed down from mines.

* Elementary Treatise on Mineralogy, according to the Principles of Prof. Werner. Vol. II.

were

were the primitive origin of their dissemination amid the strata, it certainly could have happened only at some very remote period of the grand disruptions, that have taken place on the surface and exterior strata of our globe. But these revolutions, of which we have no records, are buried in the night of time. For we shall see, that strata, which furnish gold dust, are found at a considerable depth in some hills, equally remote from mountains capable of furnishing it, and from rivers that could force it from its native situation. Consequently it could have mingled in them only at a very distant period, when the strata of the hills assumed the arrangement they have at present, namely, at the time of their formation.

This the opinion of many. Such too has been the opinion of several naturalists of our country, and I should be guilty of injustice to them, if, in collecting fresh proofs tending to support their hypothesis, I omitted the mention of their valuable works. Accordingly I shall quote Mr. de Robillant, who, speaking of the gold dust found in the sands of the Orco, says very positively: "this river carries along gold, which the people of the country observe only below the bridge down to the Po; which confirms the opinion held by the people best acquainted with the natural history of the country, that it is from the gullies and hills that this gold dust is washed down into the river by the rapidity of the water during storms---*. This valuable metal does not come from the high mountains, since none is found above the bridge, but it originates from the washing of the red earth, of which most of these hills and plains are composed, and which in stormy weather is carried down into the principal river†.

De Robillant.

Balbo. Mr. Balbo generally adopts the explanation of Mr. de Robillant respecting this species of native gold, in his learned Memoir on the auriferous sand of the Orco. "Every one," says he, "knows, that gold dust is collected in the Orco······But I do not believe it is equally known, that gold is found, not in the bed of the river alone, but to the

* See a geographical Essay on the continental Territories of the King of Sardinia, by de Robillant, in the Memoirs of the Royal Academy of Sciences of Turin for the years 1784,5, Part II, p. 234.

† Ib. p. 268.

distance

distance of several miles, every where mingled more or less with the sand......It is very positively asserted, that it occurs in all the little rivulets between Valperga and Rivara ......I endeavoured to discover, whether all the waters rise sufficiently near to each other, to lead us to suppose, that they equally derive their gold from the same mine: as it is in this way that the vulgar, and even most of the learned, generally account for the gold found in rivers. But I was completely convinced, that the waters of which I speak arise from different heights at some distance from one another, so that, as we cannot suppose all these places to contain mines, from which the gold may be derived, we must necessarily admit, that the particles of gold are not separated daily by the action of the water, and carried along by its streams, but that the water finds them in the soil itself over which it flows......And it is further confirmed by the observation, that the auriferous strata disappear as we proceed up the Orco; that we find them at farthest only as high as the bridge; that above this all traces of them are lost, though this is very far from the springs, while as we descend into the plain these strata are every day exposed by the action of the water, and particularly in floods *."

Found in the earth several miles round a river.

In a second part I shall speak of the theory proposed by Mr. Napion, in his Memoir on the Mountains of Canavais †, who, having observed that all the pyrites of those mountains are auriferous, attributes the particles of gold to their decomposition or attrition. This is the opinion of our worthy colleague, Dr. Bonvoisin.

Supposed to come from pyrites.

The observations I am now about to communicate appear to me still more decisive, than the proofs alleged by these authors; and if the earths of which I shall speak do not furnish so large a quantity of gold dust, they afford indisputable arguments, to convince us, that the gold certainly does not proceed from any mine traversed by water, at least in the present day.

Further proof of its origin to be adduced.

* Mem. of the Roy. Ac. of Turin for 1784,5, on the auriferous Sand of Orco, Part II, p. 404, 407.

† Ib. for 1785,6, p. 345,6.

On

Hills in the department of the Doire.

On the north of the commune of St. George's, in the circle of Chivas, in the department of the Doire, we find fertile rising grounds, and hills almost wholly covered with vineyards, which continue till we come to the highest of them, the hill of Macugnano, part of which is cultivated, part covered with wild chestnut trees; a distance of about three miles.

Three distinct strata.

In proceeding from the outer and upper surface of these hills to the bottom of the valleys, which intersect them in different directions, we find in general three very distinct strata.

The upper.

The middle.

The lower.

The upper stratum is for the most part argillaceous, as it furnishes an excellent earth for making bricks and tiles. The thickness of this stratum varies in different places from three or four feet to twenty-five or thirty. The second stratum, which stretches likewise horizontally beneath the stratum of clay, is a few feet thick. It is composed of a considerable portion of sand, of gravel, and of pebbles of different natures, argillaceous, calcareous, and quartzose. Of these I shall speak more particularly in the second part, as well as of the fragments produced by their being broken or decomposed. The third or lower stratum, which forms the bed of the valleys, and of the rivulets that run through them in rainy weather, is composed in great measure of the fragments of the argillaceous and calcareous stones of the second stratum.

Valleys produced,

and gold found in them.

The rains gradually produced little gullies in different directions; which by the falling of fresh rain, and the quantity and rapidity of the water, have in the course of time been extended and converted into valleys, more or less broad and deep, in different places. Part of the water of several gullies accumulates particularly in one valley, where during storms and long rains it forms a torrent, called in the country the Merdanzone. Now the gold dust is found chiefly among the sands of this torrent, and of the small lateral rivulets, that flow into the Merdanzone or other similar valleys.

None in the

But does this gold proceed equally from the different strata I have mentioned above, or from one of them only? I first examined the brick earth, that of the upper stratum, in

in different places and at various depths: I also examined considerable depositions of this earth accumulated in the shallow valleys: but I never discovered the smallest particle of gold in it. The searchers for gold knows this so well by long experience and a great number of fruitless trials, that they never pay any regard to this stratum. It is the stratum beneath this, that composed of gravel, sand, and broken stones, in which the particles of gold are found. upper stratum but in the middle one.

Of this I have convinced myself by repeated trials: and though in general, if equal quantities of earth be taken from this stratum, and from the bottom of the torrent or rivulets flowing into it, the latter will yield most gold, it scarcely ever happens, that no gold is found in the latter upon trial. The particles of gold obtained from the auriferous stratum itself, which have not yet been rolled along with the sand by the rains, have a duller and deeper yellow colour than those collected in the bed of the torrent, or of the rivulets, which are of a more shining yellow, no doubt in consequence of the attrition. They are generally found amid a sand, that is more or less fine and blackish, and apparently of a siliceous and ferruginous nature. The earth of the same nature, which reaches to some distance, equally contains gold. Thus a brook that runs on the east of the commune of Aglie, between the mansion and the park, and receives the rain water that washes down an earth composed of different strata of the same nature as those of the auriferous hills of St. George's, equally rolls along particles of gold disseminated beneath the argillaceous stratum, which in certain places is of very considerable thickness. Most in the beds of rivulets. This distinguishable from the other.

Fifteen or twenty years ago several persons in the commune of St. George's made it their principal employment, to search for gold in the sand of the torrents and rivulets that I have mentioned. This they did particularly after or during heavy rains, and after storms. Formerly collected

The quantity of gold they collected in a day was very variable. Sometimes each of them would gain eight or ten shillings a day, at other times scarce a fourth or fifth of this sum. The size of the particles too varied much, as from an almost invisible atom to the weight of nine or ten grains or with some profit.

more

more. They were afterward sold to merchants, who sent them to the mint.

Gold found in other situations.

I am not speaking here, as is obvious, of gold dust disseminated in arable land. Earth of this kind in the territory of Salussole, as I am informed by my colleague Giobert, contains particles of gold. The earth of gardens is known to contain them. It has been proved in our days by the experiments of Sage, Berthollet, Rouelle, Darcet, and Deyeux, that there are particles of gold in vegetables. Berthollet has extracted about 2·14 gram. [33 grs.] from 48900 gram. or a hundred weight of ashes.

Here not on the surface, but under it, sometimes to a considerable depth.

Hitherto gold has not been found in the arable land in the environs of St. George's, but only in the stratum beneath the clay, the surface of which is cultivated. The auriferous stratum, as I have observed, is more than thirty feet deep below the argillaceous stratum in some places.

We have nothing to do here with particles of gold mixed with the surface mould by the decomposition of plants, or which plants have derived from the earth. I have no doubt, that the particles of gold found in the environs of St. George's have the same origin as those met with from Pont to the entrance of the Orco and of the Mallon into the Po, from Valperga and Rivara to Aglie and St. George's; as well as of those, which Dr. Bonvoisin observed in the environs of Challant in the valley of Aoste. The famous piece of native gold preserved in the arsenal was found there. In that space pieces of gold of the weight of a louis have sometimes been found; and other pieces are mentioned of the value of more than 100 l. [£4 3s. 4d.]. It is probable, that the gold found in the earth in the valley of Brozzo, and in other places, has the same origin. I shall propose my conjectures on this subject in the second part of this memoir, where I shall enter more at large into the nature of the earths and stones of the auriferous strata, as well as the nature of the land in which they are contained.

Every where it has a common origin.

IV.

IV.

Calculation of the direct Attraction of a Spheroid, and Demonstration of CLAIRAUT's *Theorem. By a Correspondent.*

To Mr. NICHOLSON.

SIR,

THE same mode of calculation, by which the figure of a gravitating body, differing but little from a sphere, has been determined (p. 208 of this volume), is also applicable to the magnitude of its immediate attraction, or the comparative length of a pendulum in different latitudes. Extension of former researches.

Suppose a sphere to be inscribed in the spheroid, and another to be circumscribed about it; I shall first show, that the attraction at the pole is equal to that of the smaller sphere increased by $\frac{4}{15}$ of that of the shell, and at the equator equal to that of the larger diminished by $\frac{2}{15}$. If we call the attraction of this shell 2, its surface being equal to the curved surface of a circumscribing cylinder, the attraction of a narrow ring of this cylinder, or of the elevated portion of the spheroid at the equator, supposed to act at the distance of the radius, or unity, may be expressed by its breadth: but in its actual situation its attraction in the direction of the axis is reduced in the ratio of the cube of the chord of half a right angle to the cube of the radius; and the attraction of any other ring will be to this in the ratio of the quantity of matter, or the cube of the sine of the distance from the pole, and of the versed sine directly, and in the ratio of the cube of the chord inversely; that is in the joint ratio of the cube of the cosine of half the angle and the versed sine: thus, if we call the cosine of half the angle x, the versed sine being $2-2x^2$, and the fluxion of the arc $\frac{2\dot{x}}{\sqrt{(1-xx)}}$, the fluxion of the force at the equator will be $\frac{1}{2\sqrt{2}} \cdot \frac{2\dot{x}}{\sqrt{(1-xx)}}$, and elsewhere as much less as Attraction of the prominent parts.

$x^3(2-2x^2)$ is less than $\frac{1}{2\sqrt{2}}$, that is, $\frac{4x^3\dot{x}}{\sqrt{(1-xx)}} - \frac{4x^5\dot{x}}{\sqrt{(1-xx)}}$, of which the fluent is found as before $(\frac{4}{5}x^4 - \frac{4}{15}x^2 - \frac{8}{15})\sqrt{(1-xx)}$; and this becomes $\frac{8}{15}$ while x increases from 0 to 1, being to 2, the attraction of the whole shell, as $\frac{4}{15}$ to 1; but if the radius of the sphere be 1, and the ellipticity e, the attraction of the shell will be to that of the sphere as $\frac{3e}{n}$ to 1, n being the mean density of the sphere, compared with that of the superficial parts, and the attraction of the spheroidal prominence will be expressed by $\frac{4e}{5n}$, that of the sphere being unity.

Polar & equatorial attraction. The depression below the circumscribed sphere is equal, on the meridian, to the elevation above the inscribed sphere: but vanishes at the equator, being every where proportional to the square of the sine of the latitude; so that the mean depression of each of an infinite number of rings, of which any point of the equator is the pole, must be half as great as the elevation of the corresponding rings parallel to the equator; and the whole deficiency is equal to half of the whole excess, that is, to $\frac{2e}{5n}$: consequently, the remaining attraction of the shell is $\frac{13e}{5n}$, from which we must deduct the diminution of the attraction of the inscribed sphere $2e$, and the whole will become $1 + \frac{13e}{5n} - 2e$, which subtracted from $1 + \frac{4e}{5n}$ leaves $2e - \frac{9e}{5n}$ for the excess of the immediate attraction at the pole above the equatorial attraction; to which if we add the centrifugal force f, the whole diminution of gravity g will be $2e - \frac{9e}{5n} + f$; but since e was before found to f as 1 to $2 - \frac{6}{5n}$ or $e = \frac{5n}{10n-6} \cdot f$, we have $\frac{10n-9}{5n} \cdot e = \frac{10n-9}{10n-6} \cdot f$,

and

and $g = \frac{20\,n - 15}{10\,n - 6} \cdot f$, to which if we add e, we find $e + g = \frac{25\,n - 15}{10\,n - 6} \cdot f = \frac{5}{2} f$; and this is the celebrated theorem of Clairaut.

It remains to be shown, that the diminution of the attractive force at different parts of the spheroid varies as the square of the cosine of the latitude. The elevation, being every where proportional to the square of the distance from the axis, may be divided into two parts; one proportional to the square of the sine of the distance from the meridian of the place, and the other to the distance from the plane of another meridian perpendicular to it: but the first of these being constant, whatever may be the position of the place to be considered, the second only produces the variation. Now if we take in the second portion the mean of the elevations at any two points of a less circle equidistant from the meridian, it will be proportional to the sum of the squares of the distance of the centre of the circle from the axis, and of the cosine of the distance from the meridian in the same circle, reduced to a similar direction, that is, diminished in the ratio of the radius to the sine of the latitude, since twice the sum of the squares of any two quantities is equal to the sum of the squares of their sum and their difference. We have therefore two quantities, varying as the square of the cosine, and as the square of the sine of the latitude respectively: but the square of the sine may be represented by a constant quantity diminished by the square of the cosine: and the decrease of the attraction of the inscribed sphere is as the elevation, which is as the square of the cosine; the centrifugal force reduced to a vertical direction is also as the square of the cosine. We have therefore, beside two constant quantities, two negative forces and a positive one, all varying as the squares of the cosine of the latitude; and it is obvious, that the joint result of the whole, or the upper real diminution of gravity, must also vary in the same proportion.

Variation of gravity.

A. B. C. D.

29 *June*, 1808.

V.

V.

Reply to Professor VINCE's *Ultimatum. By a Correspondent.*

To. Mr. NICHOLSON.

SIR,

IT is no unusual expedient with an expert disputant, to affect a contempt for his antagonist, which he does not feel; and to decline a contest, to which he is unequal, on the pretence, that it is superfluous to engage in it. I am far from wishing, to protract a controversy with Professor Vince; but I protest against his right to excuse himself from the necessity of replying to any future observations of mine on the ground of his engagement not to trouble you further on the subject. If however the Professor thinks my remarks undeserving of any additional notice from himself, it is to be presumed, that some person will be found among the numerous disciples of that illustrious school, in which he holds so distinguished a situation, who will undertake the easy task of confuting me, and vindicating the honour of the university from the slightest shade, that the publicity of such a mistake as I have imputed to Professor Vince could possibly cast on it.

Silence not to be allowed as an argument.

I grant, that *three quantities* are "put down" in the essay, which the Professor *now calls* the first terms of three series: but I still shall deny, that these series are to be found, or are any where mentioned, in that work. It is not a little remarkable, that a man, whose life is devoted to the science of reasoning with accuracy, should adduce so weak a proof of my being guilty of misrepresentation.

Series not mentioned.

With respect to the sufficiency of each of these quantities for determining its share of the force, Professor Vince's words are, p. 19: "Now the terms omitted in the series are comparatively so extremely small, that if they were not considered, they could make no sensible alteration in the result." And now he accuses me of a second "unaccountable misrepresentation," for saying, that he has mentioned

Quantities employed said to be sufficient.

tioned the terms actually employed "as sufficient" for his purpose.

In the third place, he does not appear to be aware of the distinction between physical and mathematical accuracy. Physically speaking, the series "*may certainly*" vary as $\frac{1}{a^2}$, with as little sensible errour as the law of gravitation: mathematically speaking, we have not the slightest evidence, that the law of gravitation "*varies accurately*" as $\frac{1}{a^2}$, and in this sense the Professor's assertion is totally void of foundation.

Law of gravitation not a mathematical truth.

The change of the law of density of the medium at the surface of a planet, instead of being "inconsistent with Newton's hypothesis," is the simple and unavoidable consequence of it. Each particle of matter being supposed to induce a certain state of the medium around it independently of all others, so that the attraction may be produced alike in all circumstances, the state of the medium within the planet must necessarily be such, as to produce the joint effect of all the attractions; that is, the force must vary as $x\dot{x}$, and the density as xx or aa; the square of the distance from the centre; and this must be the immediate consequence of the same cause, that produces the usual variation of density with respect to a single particle. It may be said, that the operation of this cause is equally obscure with the ultimate effect of gravitation considered as independent of it; and I am perfectly ready to admit the objection. I am not defending the Newtonian hypothesis; I am only endeavouring to show, that Professor Vince has attacked it unsuccessfully, and has heaped errour upon errour in attempting to support his arguments.

Law of density necessarily changed within a planet.

I am, Sir,

Your very obedient servant,

'h *July*, 1808. DYTISCUS.

VI.

VI.

Question respecting the Ignition of Tinder by compressed Air. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Combustible substances ignited by condensed air,

AMONG the number of philosophical apparatus of modern invention, there are perhaps few which involve more interesting matter of inquiry, than an instrument lately contrived for setting fire to combustible substances by the agency of compressed air.

In a syringe of small size.

The little apparatus, which I have seen for this purpose, was in the possession of Mr. Accum, who showed me its surprising effects in igniting common tinder, and different species of fungi. This singular mode of producing fire is accomplished by the quick compression of the small quantity of air contained in a condensing syringe of small size.

What is the cause?

It might perhaps be matter of considerable importance, in a philosophical point of view, to ascertain what change the air undergoes during this operation; whether the effect produced is to be ascribed to the mechanical action of the air, or to a change of capacity induced by the rapid condensation?

Your remarks may tend to the elucidation of this very curious fact.

I am, Sir,

Yours respectfully,

Lincoln's Inn, July 15th, 1808. T. CLIFTON.

REPLY.

Apparently diminution of capacity.

This experiment, which is now of some standing, seems to depend on the diminution of capacity, produced by the sudden

sudden condensation. From Mr. Dalton's experiments, (see our Journal, vol. III, p. 160) it appears, that the condensation answering to the pressure of our atmosphere affords an increase of temperature upwards of 50 degrees; and, if we suppose this augmentation to be in the simple ratio of the compression, though it is probably higher, a compression of 18 atmospheres would give the temperature of ignition.

W. N.

VII.

Description of a portable artificial Horizon for taking Altitudes at Sea or Land, by Mr. WRIGHT. *In a Letter from the Inventor.*

SIR,

I Beg leave to transmit you a description and delineation of an artificial horizon, which I conceive will be found preferable to any other in use. Artificial horizon described.

Plate VII, fig. 1, represents the horizon with all its parts complete: fig. 2, a cylindric vessel of brass, to be filled with water when in use: fig. 3, the upper part of the horizon taken from the vessel, to show its internal parts. AA are two uprights of brass and a horizontal axis on the top, with two fine edges at BB, on which the brass frame CC is suspended. At the bottom of the frame are an index and two sights; the nearest sight E having a fine horizontal edge on its top, and on the farthest sight F is a fine black line in a piece of transparent ivory. The index is adjusted to a horizontal position by two screws, which fasten it also to the frame when adjusted; on the index is a convex glass to magnify the line on the ivory sight F, and throw its image on the edge of the sight E; and under the index, in its centre, is screwed a thin brass blade H, to be immersed in the water in the vessel, fig. 2, for the purpose of preventing the horizon from getting any vibrating or pendulous motion

to

to disturb its gravity, or divert the sights from their horizontal direction, when in use.

If the ship have a considerable motion, it is advisable, to suspend the box by a small brass gimmal at the top to a portable stand; and to prevent the wind's affecting it, a glass slides into each end of the box, through which the observation is taken; their surfaces being parallel, you adjust it by the sea horizon, or by meridian altitudes of the sun, the latitude being known, or by any other method observers make use of, or allow the index errour, as is done with the octants and sextants.

D is a small brass pin, to prevent the index from getting any motion in carriage; and is to be taken out, when the horizon is in use.

In observing the moon, planets, or stars, by night, a small lanthorn with a lamp is necessary, to be placed behind the box, so that the light may fall on the ivory, to show the line distinctly; and to prevent its spreading too much when you are observing the stars, the glass is to be taken out, and the brass with a small square hole, G, slid into its place.

Method of taking an altitude with it.

To take an altitude with the octant and artificial horizon, bring the eye as near to the horizon in the box as the frame of the octant will admit of, and in a horizontal line look at the fine edge of the sight E, which by the least motion of the head you may bring into contact with the line on the ivory sight F, and move forward the index on the frame of the octant with your hand, to bring the object you are observing to a coincidence with the ivory line also.

For altitudes of the sun or moon, and for all terrestrial objects, an octant of the usual construction will answer every purpose; but for observing the stars, one with a larger horizon glass, and its silvered surface also larger, with a different sight vane, would be preferable, and prevent your missing or mistaking a star when near to others, or its getting out of the glass in bringing into contact with the horizon. With such a quadrant and this horizon, the meridian altitudes of all bright stars, as they come to the meridian, may be taken, by which means the latitude might be frequently found by observations at night, and with as much ease as by the sun at noon-day; also, the altitudes of the moon

Mr. Wright's Artificial Horizon

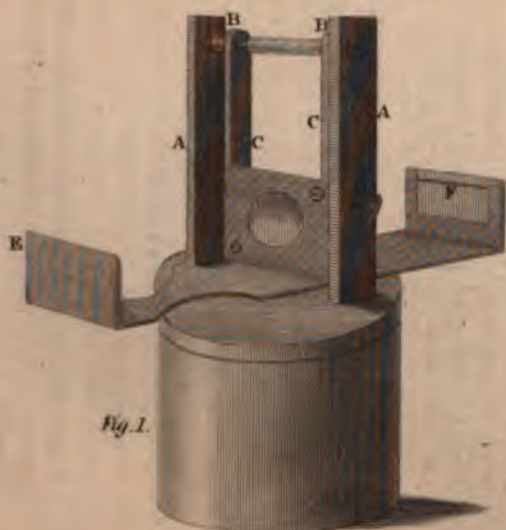


Fig. 1.

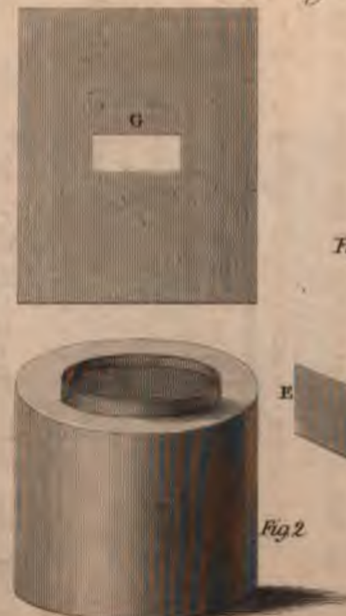


Fig. 2

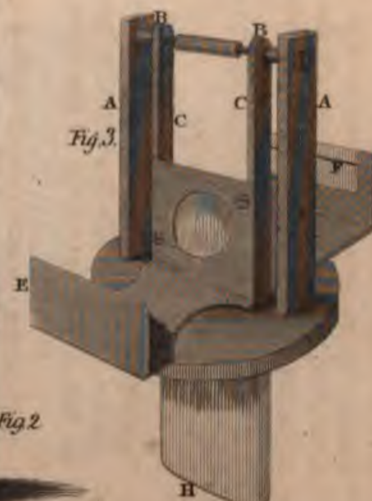


Fig. 3.

R. Roffe sc. 52 Queen Ann St. East.

moon and stars, to correct the lunar problem for the longitude, will be more correctly and easily taken with it.

I am, Sir,

Your very obedient humble servant,

J. WRIGHT.

VIII.

Description of an Apparatus to secure Persons from sinking in Water, or to act as a Life-preserver when shipwrecked, with instances of its Utility: by Mr. F. C. DANIEL, *of Wapping**.

SIR,

I HAVE taken the liberty of sending one of my life preservers, and am proud to say, they have realized the name; and I shall feel myself obliged if you will cause it to be brought before the Society for their approbation. I beg to say, Sir, though I have given it publicity, it has never been before any committee. Life-preserver

I have enclosed a copy of a letter, which I received from the only surviving officer of the Alert private ship of war, and, independent of that document, I have had information from respectable authority, that the machines have saved several lives. has already saved several lives.

It is not, Sir, a pecuniary reward I look for, although I have sunk near £1500 in the undertaking; yet, I must confess, to have the sanction of the Society of Arts would be highly flattering, and the world from that moment must be convinced of their utility.

I have the honour to be, Sir,

Your obedient servant,

F. C. DANIEL.

* Abridged from Transactions of the Society of Arts for 1807. The gold medal was voted to Mr. Daniel for this invention.

Copy

Copy of a Letter from Mr. George Willers, *late Surgeon of the Alert, private Ship of War, lost off the Western Islands.*

Sir,

Privateer

wrecked near the Western Islands.

The surgeon saved by a life-preserver.

I AM happy in having it in my power to say, I owe my life to your invaluable invention, the life preserver; and the circumstances relative thereto are as follow:—I shipped as surgeon on board the Alert, private ship of war, mounting 18 guns, and 98 men, commanded by James Desormeaux, esq., belonging to Messrs. Wright and Birch, Walbrook. We sailed from Falmouth, April 1805, and, after cruising five months, on the 22d of September, we unfortunately struck on a rock among the Western Islands, and the ship went to pieces in five minutes; at that time we had eighty-four men on board: I witnessed the loss of every officer, six in number, and sixty-four foremast men; thirteen of the crew were saved, by clinging to pieces of the wreck, spars, &c. which drifted from the wreck; and I have the happiness to say, by possessing one of your life preservers (though I cannot swim,) I was supported for some time, the sea running mountains high, but providentially a large Portuguese boat put off to my assistance, being then near a mile from the shore; and I was thus saved, by the hand of Providence and your invention, from a watery grave.

I beg, Sir, you will permit me to acknowledge how much I feel myself obliged to you; and you are at full liberty to make this case known for the benefit of mankind.

I am, Sir,
Your most obedient servant,
G. H. WILLERS.

Copy of a Letter from John Dickenson, *Esq. of the City of Norwich, to Mr.* Daniel.

Sir,

I INTENDED myself the pleasure of calling on you, and acquainting you personally of a singular incident, when the

the excellence of your machine, or life preserver, was most conspicuously manifested.

I went from the city of Norwich, in a pleasure-boat that I keep for the amusement of sailing, in company with a gentleman and two ladies. As our return to Norwich in the evening was indispensible, and the direction of the wind favouring us both ways, a few hours would effect it, the distance being only thirty miles: accordingly we set sail about four o'clock, it being moon-light during the night; and fortunately procured, in case of accident (the wind blowing hard at south-east) one of your life preservers, through the interest of a friend, of a captain, who had purchased one at Newcastle. The precaution proved, in a short time after sailing, to have been a fortunate one indeed. On tacking to enter Norwich river, at the extremity of a broad water, two miles over, known by the name of Braydon, a sudden gust overset the boat, precipitating myself, companion, and two ladies, into as agitated a water as I have ever seen at sea, (except in hard blowing weather). You may judge my situation at such a juncture. Your machine was jokingly filled as we came along, to which I ascribe (though very unexpected by us) our preservation. The gentleman, whose name is Goring, was inexpert at swimming, and with difficulty kept himself up, till I reached him; and then directing him to lay hold of the collar of my coat, over which the machine was fixed, I proceeded towards the ladies, whose clothes kept them buoyant, but in a state of fainting when I reached them: then taking one of the ladies under each arm, with Mr. Goring hanging from the collar of the coat, the violence of the wind drifted us on shore upon Burgh Marshes, where the boat had already been thrown, with what belonged to her. We got the assistance of some countrymen directly, (after taking refreshment at a marsh farmer's house, where we procured some dry clothing for the ladies, who were now pretty well recovered,) and by their endeavours put the boat in sailing trim, and prosecuted our voyage to Norwich, which we effected by eleven o'clock that night.

A pleasure boat overset.

Two gentlemen and two ladies buoyed up by one life-preserver.

From this singular escape, on my return from Birmingham, I shall be induced to inspect your warehouse, and procure

procure the various prices of your invention, anxious recommend it in even sailing excursions, in which utility has been so evidently demonstrated, and its ascertained.

You are at liberty, Sir, to make whatever use you plea of this account, and I beg to subscribe myself,

Sir,

Your most obedient humble servant,

JOHN DICKENSON

Swan with Two Necks, Lad Lane,
Jan. 30, 1807.

Reference to the figure of Mr. Daniel's Machine, called Life Preserver when Shipwrecked, Pl. VIII, Fig. 1.

The machine described.

A, represents the body of the machine, which is doub throughout, made of pliable water-proof leather, lar enough to admit its encircling the body of the wear whose head is to pass betwixt the two fixed straps, B which rest upon the shoulders; the arms of the wea pass through the spaces on the outside of the straps; o on each side, admitting the machine under them to encir the body like a large hollow belt; the strap, C, on t lower part of the machine, is attached to the back of and by passing betwixt the thighs of the wearer, and buc ling at D, holds the machine sufficiently firm to the bod without too much pressure under the arms. The machin being thus fixed, is inflated with air by the wearer blowi in from his lungs, through the cock E, a sufficient quanti of air to fill the machine, which air is retained by turni the stop-cock. The machine, when filled with air, will di place a sufficient quantity of water, to prevent four perso from sinking.

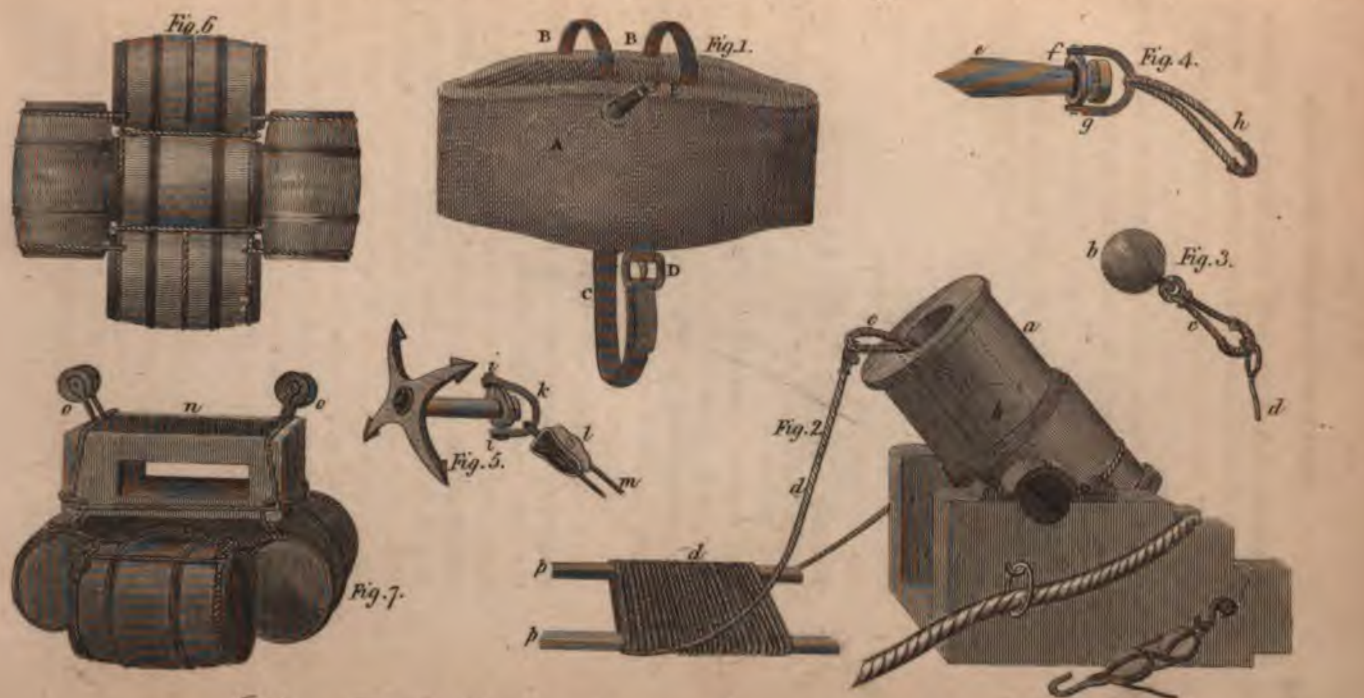
Method of making it.

Mr. Daniel recommends his life preservers to be prepar as follows: viz. To select sound German horse-hides, a to cut a piece six feet long, and two feet six inches wid free from blemish or shell; it is first to be curried, and th rendered water-proof by Mollerstein's patent varnish, of C

bor

M^r. Daniel's Life-preserver from Shipwreck.

Nicholson's Philos. Journal. Vol. XX. Pl. 8. p. 284



Lieut^t. Bell's Method of saving Persons from stranded Ships.

J. Roffe sc.

born street, Whitechapel, which preserves the leather more supple, and admits it to be easier inflated than any other water-proof leather.

The leather is to be nailed on a board, and the varnish applied upon it; it is then to be passed into an oven several times, the varnish being each time repeated, till the leather is completely covered; it is then cut in the form of a jacket, as above described, and neatly and firmly stitched; the seams and stitches are afterwards to be perfectly secured by the following black elastic varnish.

Take of gum asphaltum, two pounds; amber, half a pound; gum benzoin, six ounces; linseed-oil, two pounds; oil of turpentine, eight pounds; and lamp-black, half a pound; unite them together in an earthen vessel with a gentle heat. The machine, when properly made according to the drawing and description, resembles a broad belt, or circular girdle, composed of two folds of pliable leather attached together, and perfectly impervious to water. Varnish for the seams.

IX.

Account of Experiments made by Lieut. John Bell, *of the Royal Artillery, to ascertain the Practicability of throwing a Line to the Shore from a Vessel Stranded*.*

THE several trials made before a Committee of the Society at Woolwich, on the 29th of August 1791, of throwing a line on shore on this principle, were as follow. Experiments.

From

* Trans. of the Society of Arts for 1807, p. 136. A publicity having been recently given to some experiments off the eastern coasts of this island, for preserving lives in cases of shipwreck, by means of a rope attached to a shell thrown from a mortar; the Society deemed it incumbent on them to remind the public, that, so far back as the year 1792, a bounty of fifty guineas was given to Mr. John Bell, then sergeant, afterwards lieutenant of the Royal Regiment of Artillery, for his invention of throwing a rope on shore, by means of a shell from a mortar, on board the vessel in distress; the particulars of which were published in the tenth volume of the Society's Transactions, page 204; but a descriptive engraving

A ball carrying a line thrown 400 yards from a boat.

From a boat moored about 250 yards from shore, the shell was thrown 150 yards on shore, with the rope attached to it; the shell was of cast iron, filled with lead, it weighed 75 lbs., its diameter 8 inches; the rope in the trial was a deepsea-line, of which 160 yards weighed 18 lbs; the angle of the mortar, from which the shell was fired, was 45 degrees. By means of the line, Mr. Bell and another man worked themselves on shore upon his raft of casks; there were many kinks in the rope, which were with ease cleared by Mr. Bell, in which he was much assisted by his snatch blocks.

A second trial succeeded equally.

The second trial was repeated in a similar manner, and with equal success, the shell falling within a few yards of the former place, the gale of wind was brisk, and the water rough. The direction of the shell was nearly from north to south, and the wind blew nearly north-west.

Inch and half rope thrown 160 yards.

In the third trial, the mortar was elevated to 70 degrees; the rope attached to the shell was an inch and half tarred rope, of which every 50 yards weighed fourteen pounds and a half; the shell of the kind above mentioned. It fell 160 yards from the mortar, and buried itself about two thirds in the ground: the line or rope ran out was about 200 yards, and it required the force of three men to draw the shell out of the ground at that distance.

Two men worked themselves on shore by it.

The grommet, in all these trials, was of white three inch rope; and in all the above trials, by means of the line, two men worked themselves on shore upon the raft: each charge of powder was fifteen ounces.

A grapnel not so good.

A fourth experiment was made by firing, from the same mortar, a grapnel in a wooden case; it did not retain its hold in the ground so well as the shell, but amongst the crevices of rocks, or where the vessel is near shore, will be useful.

Grapnel with an endless rope.

A grapnel of this kind may be fired from a common cannon with an endless rope, running in a pulley or small block

engraving having been omitted at that time, it was thought expedient to insert it in the present publication, with some further particulars then omitted.

Models and Drawings of the whole apparatus are reserved in the Society's Repository, for the inspection of the public.

fixed

fixed thereto, by which a raft may be successively drawn to and from the vessel either by the persons on board the vessel, or those on shore.

Observations made by Lieutenant Bell, upon throwing a Line on Shore in Case of a Ship being stranded.

1st. From the proposed construction of the piece of ordnance, intended to throw the shot and line on shore, I suppose it will be between five and six hundred weight. Weight of the mortar.

The chamber is to contain one pound of powder, and the bore to admit a leaden ball of sixty pounds or upwards; the length of range, or distance, will depend upon the size of the line made use of; I suppose it will carry a deepsea-line between three and four hundred yards distance. Dimensions.

2d. All ships that have iron ballast may use this piece as a part of it, and then there would be only the trifling difference of casting so much of the ballast into the form of the piece; the leaden balls may likewise be used as ballast. May be used as ballast.

3d. I am of opinion, there are various ways, on board of a ship, that the mortar may be placed in a proper position for firing without a carriage expressly made for it; it may be placed upon a coil of rope, or its trunnions rested upon coins, or any thing else, whereby the muzzle can be raised so high, that the groove upon the trunnion appears vertical, as the piece in that position would be elevated nearly 45 degrees. May be used without a carriage.

4th. As I imagine all ships carry deepsea-lines, on that account I made use of it in the experiments at Woolwich; but if it should be thought too short for the distance, any other light line may be added to the length of it. Line.

5th. Supposing a ship's owner to purchase such a piece of ordnance with the leaden balls, and a block carriage; I do not think the whole would amount to more than ten or eleven pounds expense. Cost.

6th. Where a ship is driving or unmanageable near the shore, it would be proper to have the piece loaded, the line reeled upon handspikes or poles, and laid upon the deck ready The line to be coiled on poles.

ready for firing at any time it might be judged necessary. The handspikes or poles the line is reeled upon preserve it in a horizontal form; and they are not to be drawn out until the instant of firing: in this manner the line will deliver itself freely.

Raft. The five water casks should also be prepared in readiness, by lashing them together, and a seaman's chest fixed upon the top of them, having part of its ends or sides cut out, in order to let out such water as may be thrown into it by the surf. I dare undertake to land with such a float upon a lee shore any where upon the coast, when it might be deemed unsafe for a boat to make good its landing.

7th. There is every reason to conclude, that this contrivance would be very useful at all ports of difficult access both at home and abroad, where ships are liable to strike ground before they enter the harbour, as Shields Bar, and other similar situations, when a line might be thrown over the ship, which might probably be the means of saving both lives and property; and moreover, if a ship was driven on shore near such a place, the apparatus might easily be removed to afford assistance; and the whole performance is so exceedingly simple, that any person, once seeing it done, would not want any further instructions.

JOHN BELL.

Woolwich, Aug. 29, 1791.

Some farther Observations made by Lieutenant Bell, upon the Application of the Mortars intended for throwing a Line on Shore, in case of a Ship being stranded.

The mortar would answer for signals, 1st. In trading ships, this piece would answer for making signals of distress, by filling the chamber with powder, and well wadding it, as the report would be heard some miles distance at sea.

or defence. 2d. Such a gun, being accompanied with a few rounds of round and grape shot, would defend a ship much better than a longer gun, against any piratical or other hostile intentions, as, from its shortness, it would be more readily loaded and fired with a larger charge each time.

Not liable to 3d. Accidents from a gun bursting, which may arise from

from an unskilful person loading with too great a proportion of powder, are in this piece effectually guarded against, by the chamber being constructed to contain but one pound of powder, a quantity which is only about one third of the usual charge of a cannon. burst.

4th. From the small size of such a gun and carriage, it might be kept upon deck, without much inconvenience in working the ship, in order to be ready if necessity required; and when the ship is out at sea, it might then be put below. But from the number of dreadful wrecks, which so frequently happen along the coast, it certainly would be prudent to have it always upon deck when within sight of land, and particularly in stormy weather. Not inconvenient to keep on deck.

JOHN BELL.

Woolwich, Sept. 30, 1791.
To C. TAYLOR, M. D. SEC.

Reference to the Engraving of Lieutenant Bell's Method of throwing a Rope on Shore, from a stranded Vessel, Pl. VIII, Fig. 2—7.

a, Represents the mortar on its carriage; *b*, the shell shown within the mortar by dotted lines; *c*, the grommet, or double rope, which connects the shell and line; *d d*, the line to be thrown on shore, now ready wound on the poles or hand-spikes, *p p*, which are to be withdrawn when the mortar is fired. Description of the apparatus.

Fig. 3 Is a separate view of the shell, with the grommet and end of the line attached thereto, explained by the same letters.

Fig. 4 Shows another invention, suggested instead of a shell, and to be fired from a common cannon, in which *e*, is an iron pin; *f*, an iron collar and rope sliding upon it; *g*, an iron ring which turns upon two pins in the collar; *h*, is the grommet or double rope, attached to the ring, to which the line to be thrown on shore is fastened. This plan may be used where people are on shore, to assist when a line is thrown.

Fig. 5 Shows a grapnel, which may also be fired from a

 common

common cannon; the collar slides along it in the same manner as that in fig. 4, to allow the head of the pin to go down to the wadding within the cannon; *i i*, are two pins on which the ring *k* is movable; *l*, the block or pulley fastened to the ring; *m*, the endless or double line running through it.

This method may be used with great advantage, where a ship is stranded near the shore; but where a mortar is on board, the use of the shell and line is the most certain.

Fig. 6 Shows the method of forming a raft, by lashing together with ropes five empty water casks belonging to the ship.

Fig. 7. Represents the raft ready for use; the apparatus *n*, to hold the person upon it, is made from a seaman's chest with holes cut in the sides of it, to allow the person within it firmer hold, and to let out the water that may be thrown into it from the waves; *o o* are two pulleys attached to the ends of the chest, and through which the line is to run; the raft is to be ballasted underneath, to prevent it from upsetting.

The whole apparatus is so arranged as to be enclosed in a small box, as may be seen by a reference to that in the Society's possession.

X.

The Bakerian Lecture, on some new Phenomena of chemical Changes produced by Electricity, particularly the Decomposition of the fixed Alkalis, and the Exhibition of the new Substances which constitute their Bases; and on the general Nature of alkaline Bodies. By HUMPHRY DAVY, *Esq. Sec. R. S. M. R. I. A.**

Read November 19, 1807.

I. *Introduction.*

Electricity presumed capable of extending

IN the Bakerian Lecture which I had the honour of presenting to the Royal Society last year, I described a number

* Philos. Trans. for 1808, Part I, p. 1.

ber of decompositions and chemical changes produced in substances of known composition by electricity; and I ventured to conclude, from the general principles on which the phenomena were capable of being explained, that the new methods of investigation promised to lead to a more intimate knowledge than had hitherto been obtained, concerning the true elements of bodies*. our knowledge of the elements of bodies.

This conjecture, then sanctioned only by strong analogies, I am now happy to be able to support by some conclusive facts. In the course of a laborious experimental application of the powers of electro-chemical analysis to bodies, which have appeared simple when examined by common chemical agents, or which at least have never been decomposed, it has been my good fortune to obtain new and singular results. This conjecture verified.

Such of the series of experiments as are in a tolerably mature state, and capable of being arranged in a connected order, I shall detail in the following sections, particularly those which demonstrate the decomposition and composition of the fixed alkalis, and the production of the new and extraordinary bodies that constitute their bases.

In speaking of novel methods of investigation, I shall not fear to be minute. When the common means of chemical research have been employed, I shall mention only results. A historical detail of the progress of the investigation, of all the difficulties that occurred, and of the manner in which they were overcome, and of all the manipulations employed, would far exceed the limits assigned to this lecture. It is proper to state, however, that when general facts are mentioned, they are such only as have been deduced from processes carefully performed and often repeated. Novel processes only described minutely.

II. *On the Methods used for the Decomposition of the fixed Alkalis.*

The researches I had made on the decomposition of acids, and of alkaline and earthy neutral compounds, proved, that the powers of electrical decomposition were proportional to The powers of electrical decomposition.

* See Journal, Vol. XVIII, p. 321; and XIX, p. 37.

the strength of the opposite electricities in the circuit, and to the conducting power and degree of concentration of the materials employed.

Aqueous solutions of the alkalis.

In the first attempts that I made on the decomposition of the fixed alkalis, I acted upon aqueous solutions of potash and soda, saturated at common temperatures, by the highest electrical power I could command, and which was produced by a combination of Voltaic batteries belonging to the Royal Institution, containing 24 plates of copper and zinc of 12 inches square, 100 plates of 6 inches, and 150 of 4 inches square, charged with solutions of alum and nitrous acid; but in these cases, though there was a high intensity of action, the water of the solutions alone was affected, and hidrogen and oxigen disengaged with the production of much heat and violent effervescence.

Potash in fusion,

The presence of water appearing thus to prevent any decomposition, I used potash in igneous fusion. By means of a stream of oxigen gas from a gasometer applied to the flame of a spirit lamp, which was thrown on a platina spoon containg potash, this alkali was kept for some minutes in a strong red heat, and in a state of perfect fluidity. The spoon was preserved in communication with the positive side of the battery of the power of 100 of 6 inches, highly charged; and the connection from the negative side was made by a platina wire.

connected with the positive side.

Appeared to be a powerful conductor.

By this arrangement some brilliant phenomena were produced. The potash appeared a conductor in a high degree, and as long as the communication was preserved, a most intense light was exhibited at the negative wire, and a column of flame, which seemed to be owing to the developement of combustible matter, arose from the point of contact.

Flame emitted.

Connected with the negative side.

When the order was changed, so that the platina spoon was made negative, a vivid and constant light appeared at the opposite point: there was no effect of inflammation round it; but aeriform globules, which inflamed in the atmosphere, rose through the potash.

The platina acted upon.

The platina, as might have been expected, was considerably acted upon: and in the cases when it had been negative, in the highest degree.

The

The alkali was apparently dry in this experiment; and it seemed probable, that the inflammable matter arose from its decomposition. The residual potash was unaltered; it contained indeed a number of dark gray metallic particles, but these proved to be derived from the platina.

Inflammable matter from the decomposition of the alkali.

I tried several experiments on the electrization of potash rendered fluid by heat, with the hopes of being able to collect the combustible matter, but without success; and I only attained my object, by employing electricity as the common agent for fusion and decomposition.

Though potash, perfectly dried by ignition, is a nonconductor, yet it is rendered a conductor by a very slight addition of moisture, which does not perceptibly destroy its aggregation; and in this state it readily fuses and decomposes by strong electrical powers.

A slight addition of moisture necessary.

A small piece of pure potash, which had been exposed for a few seconds to the atmosphere, so as to give conducting power to the surface, was placed upon an insulated disc of platina, connected with the negative side of the battery of the power of 250 of 6 and 4, in a state of intense activity; and a platina wire, communicating with the positive side, was brought in contact with the upper surface of the alkali. The whole apparatus was in the open atmosphere.

Potash exposed to the air a few seconds and the apparatus in the open air.

Under these circumstances a vivid action was soon observed to take place. The potash began to fuse at both its points of electrization. There was a violent effervescence at the upper surface; at the lower, or negative surface, there was no liberation of elastic fluid; but small globules having a high metallic lustre, and being precisely similar in visible characters to quicksilver, appeared, some of which burnt with explosion and bright flame, as soon as they were formed, and others remained, and were merely tarnished, and finally covered by a white film which formed on their surfaces.

Globules like quicksilver formed.

These globules numerous experiments soon showed to be the substance I was in search of, and a peculiar inflammable principle, the basis of potash. I found that the platina was in no way connected with the result, except as the medium for exhibiting the electrical powers of decomposition; and

These the inflammable base of potash.

and a substance of the same kind was produced, when pieces of copper, silver, gold, plumbago, or even charcoal were employed for completing the circuit.

Produced in vacuo.

The phenomenon was independent of the presence of air; I found that it took place when the alkali was in the vacuum of an exhausted receiver.

In a glass tube soon dissolved the glass.

The substance was likewise produced from potash fused by means of a lamp, in glass tubes confined by mercury, and furnished with hermetically inserted platina wires, by which the electrical action was transmitted. But this operation could not be carried on for any considerable time; the glass was rapidly dissolved by the action of the alkali, and this substance soon penetrated through the body of the tube.

Soda not so easily decomposed.

Soda, when acted upon in the same manner as potash, exhibited an analogous result; but the decomposition demanded greater intensity of action in the batteries, or the alkali was required to be in much thinner and smaller pieces. With the battery of 100 of 6 inches in full activity I obtained good results from pieces of potash weighing from 40 to 70 grains, and of a thickness which made the distance of the electrified metallic surfaces nearly a quarter of an inch; but with a similar power it was impossible to produce the effects of decomposition on pieces of soda of more than 15 or 20 grains in weight, and that only when the distance between the wires was about $\frac{1}{8}$ or $\frac{1}{10}$ of an inch.

Its base solid at a lower heat and like silver.

The substance produced from potash remained fluid at the temperature of the atmosphere at the time of its production; that from soda, which was fluid in the degree of heat of the alkali during its formation, became solid on cooling, and appeared to have the lustre of silver.

Soda sometimes exploded.

When the power of 250 was used with a very high charge for the decomposition of soda, the globules often burnt at the moment of their formation, and sometimes violently exploded and separated into smaller globules, which flew with great velocity through the air in a state of vivid combustion, producing a beautiful effect of continued jets of fire.

III.

III. *Theory of the Decomposition of the fixed Alkalis; their Composition, and Production.*

As in all decompositions of compound substances, which I had previously examined, at the same time that combustible bases were developed at the negative surface in the electrical circuit, oxigen was produced, and evolved or carried into combination at the positive surface, it was reasonable to conclude, that this substance was generated in a similar manner by the electrical action upon the alkalis; and a number of experiments made above mercury, with the apparatus for excluding external air, proved that this was the case. — Oxigen always produced at the positive surface.

When solid potash, or soda in its conducting state, was included in glass tubes furnished with electrified platina wires, the new substances were generated at the negative surfaces; the gas given out at the other surface proved by the most delicate examination to be pure oxigen; and unless an excess of water was present, no gas was evolved from the negative surface. — This the case with the alkalis.

In the synthetical experiments, a perfect coincidence likewise will be found. — Confirmed by synthesis.

I mentioned, that the metallic lustre of the substance from potash immediately became destroyed in the atmosphere, and that a white crust formed upon it. This crust I soon found to be pure potash, which immediately deliquesced, and new quantities were formed, which in their turn attracted moisture from the atmosphere, till the whole globule disappeared, and assumed the form of a saturated solution of potash*. — Base of potash soon converted into potash in the air.

When globules were placed in appropriate tubes containing common air or oxigen gas confined by mercury, an ab- — The bases converted into alkali by oxigen.

* Water likewise is decomposed in the process. We shall hereafter see, that the bases of the fixed alkalis act upon this substance with greater energy than any other known bodies. The minute theory of the oxidation of the bases of the alkalis in the free air is this:—oxigen gas is first attracted by them, and alkali formed. This alkali speedily absorbs water. This water is again decomposed. Hence, during the conversion of a globule into alkaline solution, there is a constant and rapid disengagement of small quantities of gas. — Water decomposed in the process.

sorption

sorption of oxigen took place; a crust of alkali instantly formed upon the globule; but from the want of moisture for its solution, the process stopped, the interior being defended from the action of the gas.

Base of soda. With the substance from soda, the appearances and effects were analogous.

Heated in oxigen. When the substances were strongly heated, confined in given portions of oxigen, a rapid combustion with a brilliant white flame was produced; and the metallic globules were found converted into a white and solid mass, which in the case of the substance from potash was found to be potash, and in the case of that from soda, soda.

Nothing emitted. Oxigen gas was absorbed in this operation, and nothing emitted which affected the purity of the residual air.

Alkalis produced. The alkalis produced were apparently dry, or at least contained no more moisture than might well be conceived to exist in the oxigen gas absorbed; and their weights considerably exceeded those of the combustible matters consumed.

The processes on which these conclusions are founded will be fully described hereafter, when the minute details which are necessary will be explained, and the proportions of oxigen, and of the respective inflammable substances, which enter into union to form the fixed alkalis, will be given.

Evidence of being a compound of oxigen and a base, the same as with other combustible matters. It appears then, that in these facts there is the same evidence for the decomposition of potash and soda into oxigen and two peculiar substances, as there is for the decomposition of sulphuric and phosphoric acids and the metallic oxides into oxigen and their respective combustible bases.

In the analytical experiments, no substances capable of decomposition are present but the alkalis and a minute portion of moisture; which seems in no other way essential to the result, than in rendering them conductors at the surface: for the new substances are not generated, till the interior, which is dry, begins to be fused; they explode when in rising through the fused alkali they come in contact with the heated moistened surface; they cannot be produced from crystallized alkalis, which contain much water; and

and the effect produced by the electrization of ignited potash, which contains no sensible quantity of water, confirms the opinion of their formation independently of the presence of this substance.

The combustible bases of the fixed alkalis seem to be repelled as other combustible substances, by positively electrified surfaces, and attracted by negatively electrified surfaces, and the oxigen follows the contrary order*; or, the oxigen being naturally possessed of the negative energy, and the bases of the positive, they do not remain in combination, when either of them is brought into an electrical state opposite to its natural one. In the synthesis, on the contrary, the natural energies or attractions come in equilibrium with each other; and when these are in a low state at common temperatures, a slow combination is effected; but when they are exalted by heat, a rapid union is the result; and as in other like cases with the production of fire.—A number of circumstances relating to the agencies of the bases of the alkalis will be immediately stated, and will be found to offer confirmations of these general conclusions.

IV. *On the Properties and Nature of the Basis of Potash.*

After I had detected the bases of the fixed alkalis, I had considerable difficulty to preserve and confine them so as to examine their properties, and submit them to experiments; for, like the *alkahests* imagined by the alchemists, they acted more or less upon almost every body to which they were exposed.

Difficult to preserve and confine the bases.

The fluid substance among all those I have tried, on which I find they have least effect, is recently distilled naphtha.—In this material, when excluded from the air, they remain for many days without considerable changing, and their physical properties may be easily examined in the atmosphere, when they are covered by a thin film of it.

Naphtha least affected by them.

The basis of potash at 60° Fahrenheit, the temperature in which I first examined it, appeared, as I have already mentioned, in small globules possessing the metallic lustre, opacity, and general appearance of mercury; so that when

Base of potash at 60° F. resembles mercury.

* See Bakerian Lecture 1806, p. 28 Phil. Trans. for 1807, or Journal, Vol. XIX, p. 41.

a glo-

a globule of mercury was placed near a globule of the peculiar substance, it was not possible to detect a difference by the eye.

But its fluidity imperfect below 100°.

At 60° Fahrenheit it is however only imperfectly fluid, for it does not readily run into a globule, when its shape is altered; at 70° it becomes more fluid; and at 100° its fluidity is perfect, so that different globules may be easily made to run into one.

At 50° soft and malleable.

At 50° Fahrenheit it becomes a soft and malleable solid, which has the lustre of polished silver; and at about the freezing point of water it becomes harder and brittle, and when broken in fragments, exhibits a crystallized texture, which in the microscope seems composed of beautiful facets of a perfect whiteness and high metallic splendour.

At 32° brittle.

Distilled without change.

To be converted into vapour, it requires a temperature approaching that of the red heat; and when the experiment is conducted under proper circumstances, it is found unaltered after distillation.

A perfect conductor of electricity,

It is a perfect conductor of electricity. When a spark from the Voltaic battery of 100 of 6 inches is taken upon a large globule in the atmosphere, the light is green, and combustion takes place at the point of contact only. When a small globule is used, it is completely dissipated with explosion, accompanied by a most vivid flame, into alkaline fumes.

and of heat.

It is an excellent conductor of heat.

Its specific gravity

Resembling the metals in all these sensible properties, it is however remarkably different from any of them in specific gravity; I found that it rose to the surface of naphtha distilled from petroleum, and of which the specific gravity was ·861; and it did not sink in double distilled naphtha, the specific gravity of which was about ·77°, that of water being considered as 1. The small quantities in which it is produced by the highest electrical powers, rendered it very difficult to determine this quality with minute precision. I endeavoured to gain approximations on the subject by comparing the weights of perfectly equal globules of the basis of potash and mercury. I used the very delicate balance of the Royal Institution, which when loaded with the quantities I employed, and of which the mercury never exceeded ten

ten grains, is sensible at least to the $\frac{1}{1000}$ of a grain. Taking the mean of 4 experiments, conducted with great care, its specific gravity at 62° Fahrenheit is to that of mercury as 10 to 223, which gives a proportion to that of water nearly as 6 to 10; so that it is the lightest fluid body known. In its solid form it is a little heavier, but even in this state, when cooled to 40° Fahrenheit, it swims in the double distilled naphtha.

about ·6 when fluid, and less than ·77 when solid.

The chemical relations of the basis of potash are still more extraordinary than its physical ones.

Its chemical relations more extraordinary.

I have already mentioned its alkalization and combustion in oxigen gas.—It combines with oxigen slowly, and without flame, at all temperatures that I have tried below that of its vaporization.—But at this temperature combustion takes place, and the light is of a brilliant whiteness and the heat intense. When heated slowly in a quantity of oxigen gas not sufficient for its complete conversion into potash, and at a temperature inadequate to its inflammation, 400° Fahrenheit, for instance, its tint changes to that of a red brown, and when the heat is withdrawn, all the oxigen is found to be absorbed, and a solid is formed of a grayish colour, which partly consists of potash and partly of the basis of potash in a lower degree of oxigenation,—and which becomes potash by being exposed to water, or by being again heated in fresh quantities of air.

To oxigen.

The substance consisting of the basis of potash combined with an under proportion of oxigen may likewise be formed by fusing dry potash and its basis together under proper circumstances.—The basis rapidly loses its metallic splendour; the two substances unite into a compound of a red brown colour when fluid, and of a dark gray hue when solid; and this compound soon absorbs its full proportion of oxigen when exposed to air, and is wholly converted into potash.

And the same body is often formed in the analytical experiments when the action of the electricity is intense, and the potash much heated.

The basis of potash when introduced into oximuriatic acid gas burns spontaneously with a bright red light; and a white salt, proving to be muriate of potash, is formed.

Burns in oximuriatic acid.

When

Dissolves in hot hidrogen gas.

When a globule is heated in hidrogen at a degree below its point of vaporization, it seems to dissolve in it, for the globule diminishes in volume, and the gas explodes with alkaline fumes and bright light, when suffered to pass into the air; but by cooling, this spontaneous detonating property is destroyed, and the basis is either wholly or principally deposited.

Action of water.

The action of the basis of potash on water exposed to the atmosphere is connected with some beautiful phenomena. When it is thrown upon water, or when it is brought into contact with a drop of water at common temperatures, it decomposes it with great violence, an instantaneous explosion is produced with brilliant flame, and a solution of pure potash is the result.

White ring of smoke.

In experiments of this kind, an appearance often occurs similar to that produced by the combustion of phosphuretted hidrogen; a white ring of smoke, which gradually extends as it rises into the air.

Action of water when air is excluded.

When water is made to act upon the basis of potash out of the contact of air, and preserved by means of a glass tube under naphtha, the decomposition is violent; and there is much heat and noise; but no luminous appearance, and the gas evolved when examined in the mercurial or water pneumatic apparatus is found to be pure hidrogen.

Ice.

When a globule of the basis of potash is placed upon ice, it instantly burns with a bright flame, and a deep hole is made in the ice, which is found to contain a solution of potash.

Action of water on it in the open air explained.

The theory of the action of the basis of potash upon water exposed to the atmosphere, though complicated changes occur, is far from being obscure. The phenomena seem to depend on the strong attractions of the basis for oxigen, and of the potash formed for water. The heat, which arises from two causes, decomposition and combination, is sufficiently intense to produce the imflammation. Water is a bad conductor of heat; the globule swims exposed to air; a part of it, there is the greatest reason to believe, is dissolved by the heated nascent hidrogen; and this substance, being capable of spontaneous inflammation, explodes, and communicates

communicates the effect of combustion to any of the basis that may be yet uncombined.

When a globule confined out of the contact of air is acted upon by water, the theory of decomposition is very simple, the heat produced is rapidly carried off, so that there is no ignition; and a high temperature being requisite for the solution of the basis in hidrogen, this combination probably does not take place, or at least it can have a momentary existence only. Out of the contact of air.

The production of alkali in the decomposition of water by the basis of potash is demonstrated in a very simple and satisfactory manner by dropping a globule of it upon moistened paper tinged with turmeric. At the moment that the globule comes into contact with the water, it burns, and moves rapidly upon the paper, as if in search of moisture, leaving behind it a deep reddish brown trace, and acting upon the paper precisely as dry caustic potash. Moistened turmeric paper.

So strong is the attraction of the basis of potash for oxigen, and so great the energy of its action upon water, that it discovers and decomposes the small quantities of water contained in alcohol and ether, even when they are carefully purified. Decomposes the small quantity of water in purified ether and alcohol.

In ether this decomposition is connected with an instructive result. Potash is insoluble in this fluid; and when the basis of potash is thrown into it, oxigen is furnished to it, and hidrogen gas disengaged, and the alkali as it forms renders the ether white and turbid. Ether.

In both these inflammable compounds the energy of its action is proportional to the quantity of water they contain, and hidrogen and potash are the constant result.

The basis of potash when, thrown into solutions of the mineral acids, inflames and burns on the surface. When it is plunged by proper means beneath the surface enveloped in potash, surrounded by naphtha, it acts upon the oxigen with the greatest intensity, and all its effects are such as may be explained from its strong affinity for this substance. In sulphuric acid a white saline substance with a yellow coating, which is probably sulphate of potash surrounded by sulphur, and a gas which has the smell of sulphurous acid, and which probably is a mixture of that substance with Mineral acids.

The base of potash unites with gold, silver, and copper, and

When the basis of potash is heated with gold, or silver, or copper, in a close vessel of pure glass, it rapidly acts upon them; and when the compounds are thrown into water, this fluid is decomposed, potash formed, and the metals appear to be separated unaltered.

renders fusible metal less fusible.

The basis of potash combines with fusible metal, and forms an alloy with it, which has a higher point of fusion than the fusible metal.

Its action on oily compounds.

The action of the basis of potash upon the inflammable oily compound bodies confirms the other facts of the strength of its attraction for oxigen.

On naphtha.

On naphtha colourless and recently distilled, as I have already said, it has very little power of action; but in naphtha that has been exposed to the air it soon oxidates, and alkali is formed, which unites with the naphtha into a brown soap, that collects round the globule.

On concrete oils.

On the concrete oils (tallow, spermaceti, wax, for instance), when heated, it acts slowly, coaly matter is deposited, a little gas* is evolved, and a soap is formed; but in these cases it is necessary that a large quantity of the oil be employed. On the fluid fixed oils it produces the same effects, but more slowly.

On fluid fixed oils.

On volatile oils.

By heat likewise it rapidly decomposes the volatile oils; alkali is formed, a small quantity of gas is evolved, and charcoal is deposited.

* When a globule of the basis of potash is introduced into any of the fixed oils heated, the first product is pure hidrogen, which arises from the decomposition of the water absorbed by the crust of potash during the exposure to the atmosphere. The gas evolved, when the globule is freed from this crust, I have found to be carbonated hidrogen requiring more than an equal bulk of oxigen gas for its complete saturation by explosion. I have made a great number of experiments, which it would be foreign to the object of this lecture to give in minute detail, on the agencies of the basis of potash on the oils. Some anomalies occurred which led to the inquiry, and the result was perfectly conclusive. Olive oil, oil of turpentine, and naphtha when decomposed by heat, exhibited as products different proportions of charcoal, heavy inflammable gas, empyreumatic oily matter, and water, so that the existence of oxigen in them was fully proved; and accurate indications of the proportions of their elements might be gained by their decomposition by the basis of potash. Naphtha of all furnished least water and carbonic acid, and oil of turpentine the most.

When

When the basis of potash is thrown into camphor in fusion, the camphor soon becomes blackened, no gas is liberated in the process of decomposition, and a saponaceous compound is formed; which seems to show, that camphor contains more oxigen than the volatile oils. On camphor.

The basis of potash readily reduces metallic oxides when heated in contact with them. When a small quantity of the oxide of iron was heated with it to a temperature approaching its point of distillation, there was a vivid action; alkali and gray metallic particles, which dissolved with effervescence in muriatic acid, appeared. The oxides of lead and the oxides of tin were revived still more rapidly; and when the basis of potash was in excess, an alloy was formed with the revived metal. On metallic oxides.

In consequence of this property, the basis of potash readily decomposes flint glass and green glass, by a gentle heat; alkali is immediately formed by oxigen from the oxides, which dissolves the glass, and a new surface is soon exposed to the agent. On glass.

At a red heat, even the purest glass is altered by the basis of potash: the oxigen in the alkali of the glass seems to be divided between the two bases, the basis of potash and the alkaline basis in the glass, and oxides, in the first degree of oxigenation, are the result. When the basis of potash is heated in tubes made of plate glass filled with the vapour of naphtha, it first acts upon the small quantity of the oxides of cobalt and manganese in the interior surface of the glass, and a portion of alkali is formed. As the heat approaches to redness, it begins to rise in vapour, and condenses in the colder parts of the tube; but at the point where the heat is strongest, a part of the vapour seems to penetrate the glass, rendering it of a deep red brown colour; and by repeatedly distilling and heating the substance in a close tube of this kind, it finally loses its metallic form, and a thick brown crust, which slowly decomposes water, and which combines with oxigen when exposed to air, forming alkali, lines the interior of the tube, and in many parts is found penetrating through its substance*. Its action on glass explained.

In

* This is the obvious explanation in the present state of our knowledge; Perhaps the si-

In my first experiments on the distillation of the basis of potash, I had great difficulty in accounting for these phenomena; but the knowledge of the substance it forms in its first degree of union with oxigen afforded a satisfactory explanation.

V. *On the Properties and Nature of the Basis of Soda.*

Basis of soda.

The basis of soda, as I have already mentioned, is a solid at common temperatures. It is white, opaque, and when examined under a film of naphtha, has the lustre and general appearance of silver. It is exceedingly malleable, and is much softer than any of the common metallic substances. When pressed upon by a platina blade, with a small force, it spreads into thin leaves, and a globule of the $\frac{1}{40}$th, or $\frac{1}{50}$th of an inch in diameter is easily spread over a surface of a quarter of an inch*, and this property does not diminish when it is cooled to 32° Fahrenheit.

Conducts heat and electricity.

It conducts electricity and heat in a similar manner to the basis of potash; and small globules of it inflame by the voltaic electrical spark, and burn with bright explosions.

Specific gravity ·9348.

Its specific gravity is less than that of water. It swims in oil of sassafras of 1·096, water being 1, and sinks in naphtha of specific gravity ·861. This circumstance enabled me to ascertain the point with precision. I mixed together oil of sassafras and naphtha, which combine very perfectly, observing the proportions till I had composed a fluid, in which it remained at rest above or below; and this fluid consisted of nearly twelve parts naphtha, and five of oil of sassafras, which gives a specific gravity to that of water nearly as nine to ten, or more accurately as ·9348 to 1.

Perfectly fluid at 180°.

The basis of soda has a much higher point of fusion than the basis of potash; its parts begin to lose their cohesion at

Silex of the glass altered.

ledge; but it is more than probable, that the silex of the glass likewise suffers some change, and probably decomposition. This subject I hope to be able to resume on another occasion.

Welds at common temperatures.

* Globules may be easily made to adhere and form one mass by strong pressure: so that the property of welding, which belongs to iron and platina at a white heat only, is possessed by this substance at common temperatures.

about

about 120° Fahrenheit, and it is a perfect fluid at about 180°, so that it readily fuses under boiling naphtha.

I have not yet been able to ascertain at what degree of heat it is volatile; but it remains fixed in a state of ignition at the point of fusion of plate glass. Not easily volatilized.

The chemical phenomena produced by the basis of soda are analogous to those produced by the basis of potash; but with such characteristic differences as might be well expected. Its properties analogous to those of the base of potash.

When the basis of soda is exposed to the atmosphere, it immediately tarnishes, and by degrees becomes covered with a white crust, which deliquesces much more slowly than the substance which forms on the basis of potash. It proves, on minute examination, to be pure soda. Action of the air on it.

The basis of soda combines with oxigen slowly, and without any luminous appearance, at all common temperatures; and when heated, this combination becomes more rapid; but no light is emitted, till it has acquired a temperature near that of ignition. Of oxigen.

The flame that it produces in oxigen gas is white, and it sends forth bright sparks, occasioning a very beautiful effect; in common air, it burns with light of the colour of that produced during the combustion of charcoal, but much brighter.

The basis of soda when heated in hidrogen, seemed to have no action upon it. When introduced into oximuriatic acid gas, it burnt vividly with numerous scintillations of a bright red colour. Saline matter was formed in this combustion, which, as might have been expected, proved to be muriate of soda. Of hidrogen. Of oximuriatic acid gas.

Its operation upon water offers most satisfactory evidence of its nature. When thrown upon this fluid, it produces a violent effervescence, with a loud hissing noise; it combines with the oxigen of the water to form soda, which is dissolved, and its hidrogen is disengaged. In this operation there is no luminous appearance; and it seems probable, that even in the nascent state hidrogen is capable of combining with it*. Of water.

When the basis of soda is thrown into hot water, the de- Of hot water.

* The more volatile metals only seem capable of uniting with hidrogen; a circumstance presenting an analogy.

composition is more violent, and in this case a few scintillations are generally observed at the surface of the fluid; but this is owing to small particles of the basis, which are thrown out of the water sufficiently heated, to burn in passing through the atmosphere. When, however, a globule is brought into contact with a small particle of water, or with moistened paper, the heat produced (there being no medium to carry it off rapidly) is usually sufficient for the accension of the basis.

Of alcohol and ether. The basis of soda acts upon alcohol and ether precisely in a similar manner with the basis of potash. The water that they contain is decomposed; soda is rapidly formed, and hidrogen disengaged.

Of acids. The basis of soda, when thrown upon the strong acids, acts upon them with great energy. When nitrous acid is employed, a vivid inflammation is produced; with muriatic and sulphuric acid, there is much heat generated, but no light.

When plunged, by proper means, beneath the surface of the acids, it is rapidly oxigenated; soda is produced, and the other educts are similar to those generated by the action of the basis of potash.

Of oils and naphtha. With respect to the fixed and volatile oils and naphtha in their different states, there is a perfect coincidence between the effects of the two new substances, except in the difference of the appearances of the saponaceous compounds formed: those produced by the oxidation and combination of the basis of soda being of a darker colour, and apparently less soluble.

Of oxigen. The basis of soda, in its degrees of oxidation, has precisely similar habits with the basis of potash.

When it is fused with dry soda, in certain quantities, there is a division of oxigen between the alkali and the base; and a deep brown fluid is produced, which becomes a dark gray solid on cooling, and which attracts oxigen from the air, or which decomposes water, and becomes soda.

The same body is often formed in the analytical processes of decomposition, and it is generated when the basis of soda is fused in tubes of the purest plate glass.

Of inflammables. There is scarcely any difference in the visible phenomena of

of the agencies of the basis of soda, and that of potash, on sulphur, phosphorus, and the metals.

It combines with sulphur in close vessels filled with the vapour of naphtha with great vividness, with light, heat, and often with explosion from the vaporization of a portion of sulphur, and the disengagement of sulphuretted hidrogen gas. The sulphuretted basis of soda is of a deep gray colour. Of sulphur.

The phosphuret has the appearance of lead, and forms phosphate of soda by exposure to air, or by combustion. Of phosphorus.

The basis of soda in the quantity of $\frac{1}{40}$ renders mercury a fixed solid of the colour of silver, and the combination is attended with a considerable degree of heat. Of mercury.

It makes an alloy with tin, without changing its colour, and it acts upon lead and gold when heated. I have not examined its habitudes with any other metals; but in its state of alloy it is soon converted into soda by exposure to air, or by the action of water, which it decomposes with the evolution of hidrogen. Of tin, lead, and gold.

The amalgam of mercury and the basis of soda seems to form triple compounds with other metals. I have tried iron and platina, which I am inclined to believe remain in combination with the mercury, when it is deprived of the new substance by exposure to air. Its amalgam with other metals

The amalgam of the basis of soda and mercury likewise combines with sulphur, and forms a triple compound of a dark gray colour. and with sulphur.

VI. *On the Proportions of the peculiar Bases and Oxigen in Potash and Soda.*

The facility of combustion of the bases of the alkalis, and the readiness with which they decomposed water, offered means fully adequate for determining the proportions of their ponderable constituent parts. Proportions of the bases to oxigen to form alkalis.

I shall mention the general methods of the experiments, and the results obtained by the different series, which approach as near to each other as can be expected in operations performed on such small quantities of materials.

For the process in oxigen gas I employed glass tubes containing small trays made of thin leaves of silver, or other noble metals, on which the substance to be burnt, after being Process to determine these.

being accurately weighed or compared with a globule of mercury equal in size*, was placed: the tube was small at one end, curved, and brought to a fine point, but suffered to remain open; and the other end was fitted to a tube communicating with a gasometer, from which the oxigen gas was introduced, for neither water nor mercury could be used for filling the apparatus. The oxigen gas was carried through the tube, till it was found that the whole of the common air was expelled. The degree of its purity was ascertained by suffering a small quantity to pass into the mercurial apparatus. The lower orifice was then hermetically sealed by a spirit lamp, and the upper part drawn out and finally closed, when the aperture was so small, as to render the temperature employed incapable of materially influencing the volume of the gas; and when the whole arrangement was made, the combination was effected by applying heat to the glass in contact with the metallic tray.

Difficulties. In performing these experiments many difficulties occurred. When the flame of the lamp was immediately brought to play upon the glass, the combustion was very vivid, so as sometimes to break the tube; and the alkali generated partly rose in white fumes, which were deposited upon the glass.

When the temperature was slowly raised, the bases of the alkalis acted upon the metallic tray and formed alloys, and in this state it was very difficult to combine them with their full proportion of oxigen; glass alone could not be employed on account of its decomposition by the alkaline bases; and porcelain is so bad a conductor of heat, that it was not possible to raise it to the point required for the process, without softening the glass.

In all cases the globules of the alkaline bases were carefully freed from naphtha before they were introduced; of course a slight crust of alkali was formed before the com-

* When the globules were very small, the comparison with mercury, which may be quickly made by means of a micrometer, was generally employed as the means of ascertaining the weight: for in this case the globule could be immediately introduced into the tube, and the weight of mercury ascertained at leisure.

bustion,

bustion, but this could not materially affect the result; and when such a precaution was not used, an explosion generally took place from the vaporization and decomposition of the film of naphtha surrounding the globule.

After the combustion, the absorption of gas was ascertained, by opening the lower point of the tube under water or mercury. In some cases the purity of the residual air was ascertained, in others the alkali formed in the tray was weighed.

From several experiments on the synthesis of potash by combustion, I shall select two, which were made with every possible attention to accuracy, and under favourable circumstances, for a mean result. Two synthetical experiments on potash selected.

In the first experiment 0·12 of a grain of the basis were employed. The combustion was made upon platina, and was rapid and complete; and the basis appeared to be perfectly saturated, as no disengagement of hidrogen took place, when the platina tray was thrown into water. The oxigen gas absorbed equalled in volume 190 grain measures of quicksilver; barometer being at 29·6 inches, thermometer 62° Fahrenheit; and this reduced to a temperature of 60° Fahrenheit, and under a pressure equal to that indicated by 30 inches *, would become 186·67 measures, the weight of which would be about ·0184 grain troy †; but ·0184 : ·1384 : : 13·29 : 100; and according to this estimation 100 parts of potash will consist of 86·7 basis, and 13·3 oxigen nearly. 1st experiment.

In the second experiment ·07 grains of the basis absorbed at temperature 63° of Fahrenheit, and under pressure equal to 30·1 barometer inches, a quantity of oxigen 2d experiment.

* In the correction for temperature, the estimations of Dalton and Gay Lussac are taken, which make gasses expand about $\frac{1}{480}$ of the primitive volume for every degree of Fahrenheit.

† From experiments that I made in 1799, on the specific gravity of oxigen gas, it would appear, that its weight is to that of water as 1 to 748, and to that of quicksilver as 1 to 10142. *Researches Chem.* and *Phil.* p. 9; and with this estimation, that deducible from the late accurate researches of Messrs. Allen and Pepys on the Combustion of the Diamond almost precisely agrees, Phil. Trans. 1807, page 275; or our Journal, vol. XIX, p. 223.

equal

equal in volume to 121 grain measures of mercury, and the proper corrections being made as in the former case, this gas would weigh ·01189 of a grain.

Mean 86·1 base to 13·9 oxigen.

But ·07 + ·01189 = ·08189 : 07 :: 100 : 85·48 nearly, 100 parts of potash will consist of 85·5 of basis and 14·5 of oxigen nearly. And the mean of the two experiments will be 86·1 of basis to 13·9 of oxigen for 100 parts.

Experiment with soda.

In the most accurate experiment that I made on the combustion of the basis of soda ·08 parts of the basis absorbed a quantity of oxigen equal to 206 grain measures of mercury; the thermometer being at 56° Fahrenheit, and the barometer at 29·4; and this quantity, the corrections being made as before for the mean temperature and pressure, equals about ·02 grains of oxigen.

80 base to 20 oxigen.

And as ·08 + ·02 = ·10 : ·08, :: 100 : 80, 100 parts of soda, according to this estimation, will consist of 80 basis to 20 of oxigen.

Increase of weight indicated more oxigen

In all cases of slow combustion, in which the alkalis were not carried out of the tray, I found a considerable increase of weight; but as it was impossible to weigh them except in the atmosphere, the moisture attracted rendered the results doubtful; and the proportions from the weight of the oxigen absorbed are more to be depended on. In the experiments in which the processes of weighing were most speedily performed, and in which no alkali adhered to the tube, the basis of potash gained nearly 2 parts for 10, and that of soda between 3 and 4 parts.

but less to be depended on.

Decomposition of water by the bases.

The results of the decomposition of water by the bases of the alkalis were much more readily and perfectly obtained than those of their combustion.

Amalgam of base of potash employed.

To check the rapidity of the process, and, in the case of potash, to prevent any of the basis from being dissolved, I employed the amalgams with mercury. I used a known weight of the bases, and made the amalgams under naphtha, using about two parts of mercury in volume to one of basis.

In the first instances I placed the amalgams under tubes filled with naphtha, and inverted in glasses of naphtha, and slowly admitted water to the amalgam at the bottom of the glass; but this precaution I soon found unnecessary, for the action

action of the water was not so intense, but that the hidrogen gas could be wholly collected.

I shall give an account of the most accurate experiments made on the decomposition of water by the bases of potash and soda.

Experiment.

In an experiment on the basis of potash conducted with every attention that I could pay to the minutiæ of the operations, hidrogen gas, equal in volume to 298 grains of mercury, was disengaged by the action of ·08 of a grain of the basis of potash, which had been amalgamated with about 3 grains of mercury. The thermometer at the end of the process indicated a temperature of 56° Fahrenheit, and the barometer an atmospheric pressure equal to 29·6 inches.

Now this quantity of hidrogen* would require for its combustion a volume of oxigen gas about equal to that occupied by 154·9 grains of mercury, which gives the weight of oxigen required to saturate the ·08 of a grain of the basis of potash at the mean temperature and pressure nearly ·0151 of a grains. And ·08 + ·0151 = ·0951 : ·08 :: 100 : 84·1 nearly.

Gave 84 base to 16 oxigen.

And according to these indications 100 parts of potash consist of about 84 basis and 16 oxigen.

Experiment with base of soda.

In an experiment on the decomposition of water by the basis of soda, the mercury in the barometer standing at 30·4 inches, and in the thermometer at 52° Fahrenheit, the volume of hidrogen gas evolved by the action of ·054 of a grain of basis equalled that of 326 grains of quicksilver. Now this at the mean temperature and pressure would require for its conversion into water, ·0172 of oxigen, and ·054 + ·0172 = ·0712 : ·054 :: 100 : 76 nearly; and according to these indications, 100 parts of soda consist of nearly 76 basis, and 24 oxigen.

Gave 76 base to 24 oxigen.

Another experiment

In another experiment made with very great care, ·052 of the basis of soda were used; the mercury in the barometer was at 29·9 inches, and that in the thermometer at 58° Fahrenheit. The volume of hidrogen evolved was equal to that of 302 grains of mercury; which would demand for

* Beitraege Chem. and Phil. page 287.

its

its saturation by combustion at the mean temperature and pressure ·01549 of a grain of oxigen; and 100 parts of soda, according to this proportion, would consist nearly of 77 basis, and 23 oxigen.

gave 77 base to 23 oxigen.

Several other experiments made.

The experiments, which have been just detailed, are those in which the largest quantities of materials were employed; I have compared their results, however, with the results of several others, in which the decomposition of water was performed with great care, but in which the proportion of the bases was still more minute: the largest quantity of oxigen indicated by these experiments was, for potash 17, and for soda 26 parts in 100, and the smallest 13, and 19; and comparing all the estimations, it will probably be a good approximation to the truth, to consider potash as composed of about 6 parts basis and 1 of oxigen; and soda, as consisting of 7 basis and 2 oxigen.

From a comparison of the whole 6 base to 1 oxigen for potash and 7 base to 2 oxigen for soda probably near the truth.

(To be concluded in our next.)

XI.

Remarks on Iron Spar: by Mr. Bergman*.

Mr. Berthier found no perceptible portion of lime in iron spar.

Mr. Haüy, having been informed in a letter from Mr. Hassenfratz, that Mr. Berthier, in his analysis of iron spar, had found merely inperceptible traces of the presence of lime, sent to the laboratory of investigation belonging to the Museum two pieces of this ore, one of which was black, the other white, both regularly crystallized and free from any gangue, that they might be examined for the existence of lime. The following are the results of this preliminary examination.

Component parts of black iron spar, according to the author:

Black iron spar.

Iron, at a minimum	62
Carbonic acid united with the iron	16·9
Carbonate of lime	5
Water of crystallization	16·1
	100

* Journal des Mines, No. III, p. 241.

White

White iron spar.

Iron, at a minimum	25
Carbonic acid united with the iron	6·8
Carbonate of lime	48
Water of crystallization	17·2
Pyrites	3
	100

and of white iron spar.

After the publication of Mr. Drappier on the same subject, whose results were so different from mine, I examined anew the products, which I had carefully preserved: and accordingly I treated the 48 parts of carbonate of lime, found in the white iron spar, with weak sulphuric acid. A very brisk effervescence took place, and a very bulky magma was formed, which had all the characters of sulphate of lime. This matter, having been heated with the usual precautions to expel the moisture, was slightly calcined to drive off the excess of acid; diluted with a very small quantity of water; and filtered. The liquor had a bitter taste similar to that of sulphate of magnesia, but slightly metallic. The residuum, separated from the filter, and calcined, was perfectly white and insipid. It weighed 37 parts. If we admit 32 parts of lime in 100 of crystallized sulphate, there will be 23 in the 57 calcined: and if there be 44 parts of carbonic acid in 100 of carbonate, there must have been only 41 per cent of carbonate of lime, instead of 48 per cent mentioned above.

Mr Drappier's results very different.

Supposed carbonate of lime examined.

7 parts of it not lime,

The liquor mentioned above was left to evaporate slowly in the open air. After a few days the whole was crystallized into a white salt, that weighed 26 parts. The solution of this salt in water was very bitter, and still retained its metallic taste. On caustic potash being added, a bulky white precipitate was formed, which had the appearance of magnesia. When separated, dried, and calcined, it was of a light violet colour, owing to the presence of oxide of manganese, and weighed 5 parts. These being added to the 41 of carbonate of lime give but 2 of loss, which may be ascribed to carbonic acid belonging to the magnesia. Thus we must admit 7 per cent of carbonate of magnesia, the quantity of manganese being but very small.

but carbonate of magnesia,

The magnesia coloured with manganese was treated with radical

with a minute

portion of manganese.

radical vinegar a little diluted, and the whole was dissolved, except some traces of black oxide of manganese. The solution was slightly coloured. On heating it, it became colourless; and though the precipitate was a little increased by this ebullition, it could not be weighed on account of the smallness of its quantity.

The supposed iron oxide contained 4 parts of manganese.

As the iron might contain manganese, it was calcined with caustic potash, which thus acquired a very deep green colour. The calcination with potash was repeated, till the intensity of the colour was so far diminished, as to render it almost certain, that the whole of the manganese was separated. The alkaline liquor being saturated by an acid, the manganese was precipitated by ammonia. It weighed 4 parts. The true results therefore of the analysis of white iron spar are

Real component parts of white iron spar.

Iron	20·
Manganese	4·5
Carbonic acid united with the iron	6·8
Carbonate of lime	41
Carbonate of magnesia	7
Loss and water of crystallization	17·2
Pyrites	3
	100

Examination of the products of analysis of black iron spar.

Preceding analysis of black iron spar examined.

The five parts of carbonate of lime mentioned above, being treated in the same manner, were found to contain merely an atom of lime, the quantity of which was too small to be estimated. They consisted almost wholly of magnesia, with a little manganese. The iron too contained a perceptible quantity of manganese, which could not be separated from it completely but by repeated calcination with caustic potash.

The following alterations therefore must be made in the results of the analysis of the black iron spar.

Its real component parts.

Oxide of iron and of manganese	64
Carbonic acid united with the 2 metals	16·9
Carbonate of magnesia	3
Loss and water of crystallization	16·1
	100

XII.

XII.

Analysis of a Urinary Calculus: by Professor WURZER *.

FOR the stone I have now analysed I am indebted to Mr. Michaelis, who extracted it from a patient by the operation.

It was nearly oval, but a little flattened: brown exteriorly, and of a yellowish white within. It weighed exactly 870 grains German weight [834 grs. Eng.]. Its specific gravity was 1·572. Its surface was irregular, and a little rough. It was of the consistence of hard chalk, was without a nucleus, and composed of layers. Physical characters of the stone.

1. I macerated 300 grains of this concretion, previously powdered, in distilled water at the temperature of 12° R. [59° F.] for two days. Having filtered the liquor, it was without colour; and neither afforded any precipitate, nor was perceptibly changed, by nitrate of mercury, nitrate of silver, muriate of barytes, barytes-water, lime-water, oxalic acid, potash, or ammonia. It is evident therefore, that the distilled water had taken up none of the constituent parts of this urinary concretion. Chemical examination. Water took up nothing.

The powder when dried weighed as much as at first.

2. This powder I left for two days in muriatic acid of the specific gravity of 1·181, at a temperature of 15° R. [65·75° F.], and then added to it distilled water. After filtering, I dried the residuum thoroughly, which then weighed 248 grains, and was of a reddish brown colour. Muriatic acid took up

3. The filtered liquor, precipitated by lime-water, afforded a powder, which when collected and examined was found to be phosphate of lime. It weighed 52 grains. phosphate of lime.

4. The 248 grains that remained from the second experiment were put into a solution of potash a little diluted, and left in it for two days at a temperature of 18° R. [72·5° F.]. I then filtered off the liquor, from which acetous acid threw down a precipitate weighing 230 grains. This, carefully examined, consisted of 226 grains of uric acid, easily distinguishable by its properties and characteristics, and about 4 grains of animal matter. Potash dissolved uric acid, & some animal matter.

* Annales de Chimie, vol. LX, p. 310.

5. What

Undissolved animal matter burned

5. What remained on the filter weighed 18 grains. This I heated to incandescence in a silver crucible. During this process a very disagreeable fetid smell was emitted, resembling that of horn or hair burning. The residuum weighed scarcely 3 grains.

left 3 grains

6. These 3 grains were not soluble in sulphuric, nitric, or muriatic acid, even when heated with them in succession to ebullition.

which were silex.

7. I then mixed them with four times their weight of potash, and melted the mixture in a suitable heat. The whole dissolved in water, and I precipitated pure silex by adding an acid in excess.

This a rare occurrence.

This earth was found but twice by Messrs. Fourcroy and Vauquelin in urinary calculi, though they analysed a very great number; which induced me to repeat my operation with the 570 grains I had reserved. As I again found silex, and in a similar proportion, in these, I felt assured, that there had been no mistake in my analysis.

Found again on repeating the analysis.

From these experiments it follows, that 100 parts of this calculus contained

Uric acid	75·33
Phosphate of lime	17·35
Animal matter	6·32
Silex	1
	100.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Wernerian Natural History Society.

AT the last meeting of the Wernerian Natural History Society (July 16), the President laid before the Society three communications from Col. George Montague, F.L.S., of Knowle House, Devon. Two of these communications were read at this meeting. The first part of the first communication contained an interesting view of the natural habits and more striking external appearances of the gannet or soland goose, *pelicanus bassanus*. The second part contained an account of the internal structure of this bird, particularly of the distribution of its air-cells, which the ingenious author showed to be admirably adapted to its mode

Gannet.

mode of life, and continued residence on the water, even in the most turbulent sea, and during the most rigorous seasons. The second communication was the description and drawing of a new genus of *insect*, which inhabits the cellular membrane of the gannet, and to which Col. Montague gives the name of *cellularia bassani*.—At the same meeting, Mr. P. Neill laid before the Society a list of such fishes belonging to the four Linnean orders, apodes, jugulares, thoracici, and abdominales, as he had ascertained to be natives of the waters in the neighbourhood of Edinburgh, accompanied with valuable remarks, and illustrated by specimens of some of the rarer species. Of the *apodes* he enumerated 4 species belonging to 3 genera: 2 to muræna, 1 anarhichas, and 1 ammodytes. Of the *jugulares* he mentioned 13 species, belonging to 3 genera: 1 callionymus (the gemmeous dragonet, for, from examining many specimens, the author had concluded, that the *sordid dragonet* of Mr. Pennant and Dr. Shaw is not a distinct species, but merely the female of the gemmeous dragonet), 9 of the genus gadus, and 2 blennius. Of the *thoracici* he stated 22 species, belonging to 9 genera: 1 gobius, 2 cottus, 2 zeus, the dorée and the opah (a specimen of this last most resplendent fish having been taken off Cramond in the Firth of Forth some years ago, and being still preserved in the museum of P. Walker, Esq.), 7 pleuronectes, 1 sparus, the toothed gilt head (a rare fish, of which only two specimens have occurred in the Frith of Forth), 2 perca, 3 gasterosteus, with 1 trigla. Of the *abdominales* he had ascertained 14 species, belonging to 7 genera: 1 cobitis, 4 salmo, 3 esox, the pike, garpike, and the saury or *gandanook* (which last, though rare in England, is not, he stated, uncommon at Edinburgh, but arrives in the Frith almost every autumn in large shoals), 3 clupea. Of the genus cyprinus, of which no fewer than ten species inhabit the rivers and ponds of England (including the carp, tench, gudgeon, dace, roach, bream, &c.), only one insignificant species, the author remarked, is found near Edinburgh, viz. the common minow. Of the genus scomber, the mackarel is got in the entrance of the Frith of Forth. Mr. Neill reserved the notice of the *amphibia nantes* of Linnæus, including the ray tribe, to a future meeting.

New insect.

Fishes near Edinburgh.

Sordid dragonet, the female of the gemmeous.

METEOROLOGICAL JOURNAL
For JULY, 1808,
Kept by ROBERT BANKS, Mathematical Instrument Maker, in the STRAND, LONDON.

JUNE. Day of	THERMOMETER. 11 A. M.	11 P. M.	Highest.	Lowest.	BAROMETER.	WEATHER. Night.	Day.
29	63	64	71	56	30·14	Fair	Fair
30	63	60	69	55	30·23	Ditto	Ditto
JULY.							
1	63	60	70	56	30·16	Ditto	Ditto
2	62	61	69	56	30·06	Ditto	Ditto
3	63	60	69	51	30·02	Ditto	Rain
4	60	59	67	52	30·01	Ditto	Fair
5	61	62	67	56	29·97	Ditto	Ditto
6	61	64	68	60	30·10	Ditto	Ditto
7	68	65	72	59	30·14	Ditto	Ditto
8	68	65	72	62	30·05	Ditto	Ditto
9	64	66	71	62	30·04	Ditto	Ditto
10	65	65	72	60	30·07	Ditto	Ditto
11	68	72	76	66	30·17	Ditto	Ditto
12	74	78	83	70	30·12	Ditto	Ditto
13	80	81	87	74	30·01	Ditto	Ditto
14	82	82	87	70	30·02	Ditto	Ditto
15	76	68	81	73	30	Ditto*	Ditto
16	75	73	83	65	29·96	Ditto	Ditto
17	74	74	81	65	30·03	Ditto	Ditto
18	74	75	80	66	30·05	Ditto	Ditto
19	76	73	82	64	29·94	Ditto	Ditto
20	71	65	75	61	29·85	Ditto	Rain
21	69	65	75	60	29·75	Ditto	Fair
22	71	68	74	66	29·76	Ditto	Ditto
23	70	68	77	64	29·84	Rain	Ditto
24	73	65	81	61	29·83	Cloudy †	Rain ‡
25	65	61	68	60	29·78	Ditto	Ditto

* Lightning in the W. † Lightning in the S. E ‡ Thunder.

☞ I have lately seen in the Papers several accounts of the great height of the thermometer in various places; and as there appears much difference in the temperatures, I conceive there must have been more or less reflected heat in the different situations. The thermometers, from which I register, hang a few feet from the ground, against a wall that has nearly an eastern aspect, and is completely sheltered from the sun both at its back and front the whole day, in such a manner, that it cannot be affected by its heat, either direct or reflected. I conclude therefore, that the highest temperature here stated is a near approximation to truth.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XX.

ARTICLE I.

The Bakerian Lecture on some new Phenomena of Chemical Changes produced by Electricity; particularly the Decomposition of the fixed Alkalis, and the Exhibition of the new Substances which constitute their Bases; and on the general Nature of Alkaline Bodies. By HUMPHRY DAVY, *Esq. Sec. R.S. M.R.I.A.*

(Concluded from Page 314.)

VII. *Some general Observations on the Relations of the Bases of Potash and Soda to other Bodies.*

SHOULD the bases of potash and soda be called metals? The greater number of philosophical persons, to whom this question has been put, have answered in the affirmative. They agree with metals in opacity, lustre, malleability, conducting powers as to heat and electricity, and in their qualities of chemical combination. Are these bases to be called metals?

Their low specific gravity does not appear a sufficient reason for making them a new class; for among the metals themselves there are remarkable differences in this respect, platina being nearly four times as heavy as tellurium *; and in Their lightness not a sufficient objection.

* Tellurium is not much more than six times as heavy as the bases of soda. There is great reason to believe, that bodies of a similar

in the philosophical division of the classes of bodies, the analogy between the greater number of properties must always be the foundation of arrangement.

Nomenclature.

On this idea, in naming the bases of potash and soda, it will be proper to adopt the termination, which, by common consent, has been applied to other newly discovered metals, and which, though originally Latin, is now naturalized in our language.

Potasium and sodium.

Potasium and sodium are the names, by which I have ventured to call the two new substances: and whatever changes of theory, with regard to the composition of bodies, may hereafter take place, these terms can scarcely express an errour; for they may be considered as implying simply the metals produced from potash and soda. I have consulted with many of the most eminent scientific persons in this country upon the methods of derivation, and the one I have adopted has been the one most generally approved. It is perhaps more significant than elegant. But it was not possible to found names upon specific properties not common to both; and though a name for the bases of soda might have been borrowed from the Greek, yet an analogous one could not have been applied to that of potash, for the ancients do not seem to have distinguished between the two alkalis.

The terms should be unconnected with theory.

The more caution is necessary in avoiding any theoretical expression in the terms, because the new electro-chemical phenomena, that are daily becoming disclosed, seem distinctly to show, that the mature time for a complete generalization of chemical facts is yet far distant; and though, in the explanations of the various results of experiments that have been detailed, the antiphlogistic solution of the phenomena has been uniformly adopted, yet the motive for employing it has been rather a sense of its beauty and precision, than a conviction of its permanency and truth.

The discovery of the agencies of the gasses destroyed the hypothesis of Stahl. The knowledge of the powers and effects of the ethereal substances may at a future time possibly

similar chemical nature to the bases of potash and soda will be found of intermediate specific gravities between them and the lightest of the common metals. Of this subject I shall treat again in the text in some of the following pages.

act

act a similar part with regard to the more refined and ingenious hypothesis of Lavoisier; but in the present state of our knowledge, it appears the best approximation that has been made to a perfect logic of chemistry.

Whatever future changes may take place in theory, there seems however every reason to believe, that the metallic bases of the alkalis, and the common metals, will stand in the same arrangement of substances; and as yet we have no good reasons for assuming the compound nature of this class of bodies*. Metals not likely to be separated, and no reason yet to suppose them compounds.

The experiments in which it is said, that alkalis, metallic oxides, and earths may be formed from air and water alone, in processes of vegetation, have been always made in an inconclusive manner †; for distilled water, as I have endea- Air and water not free from solid matters.

* A phlogistic chemical theory might certainly be defended, on the idea, that the metals are compounds of certain unknown bases with the same matter as that existing in hidrogen; and the metallic oxides, alkalis, and acids, compounds of the same bases with water;—but in this theory more unknown principles would be assumed than in the generally received theory. It would be less elegant and less distinct. In my first experiments on the distillation of the bases of potash, finding hidrogen generally produced, I was led to compare the phlogistic hypothesis with the new facts, and I found it fully adequate to the explanation. More delicate researches however afterward proved, that in the cases when inflammable gasses appeared, water, or some body in which hidrogen is admitted to exist, was present. Phlogistic theory.

† The explanation of Van Helmont of his fact of the production of earth in the growth of the willow was completely overturned by the researches of Woodward. Phil. Trans. Vol. XXI. page 193. Van Helmont's experiment.

The conclusions which M. Braconnot has very lately drawn from his ingenious experiments, Annales de Chemie, Fevrier 1807, page 187, [see our Journal, vol. XVIII, p. 15.] are rendered of little avail in consequence of the circumstances stated in the text. In the only case of vegetation in which the free atmosphere was excluded, the seeds grew in white sand, which is stated to have been purified by washing in muriatic acid; but such a process was insufficient to deprive it of substances, which might afford carbon, or various inflammable matters. Carbonaceous matter exists in several stones, which afford a whitish or grayish powder; and when in a stone the quantity of carbonate of lime is very small in proportion to the other earthy ingredients, it is scarcely acted on by acids. Braconnot's experiments.

 voured

voured to show *, may contain both saline and metallic impregnations; and the free atmosphere almost constantly holds in mechanical suspension solid substances of various kinds.

All the products of living beings may be elicited from known combinations.

In the common processes of nature, all the products of living beings may be easily conceived to be elicited from known combinations of matter. The compounds of iron, of the alkalis, and earths, with mineral acids, generally abound in soils. From the decomposition of basaltic, porphyritic †, and granitic rocks, there is a constant supply of earthy, alkaline, and ferruginous materials to the surface of the earth. In the sap of all plants, that have been examined, certain neutrosaline compounds, containing potash, or soda, or iron, have been found. From plants they may be supplied to animals. And the chemical tendency of organization seems to be rather to combine substances into more complicated and diversified arrangements, than to reduce them into simple elements.

Organization rather combines than decomposes.

VIII. *On the Nature of Ammonia and alkaline Bodies in general; with Observations on some Prospects of Discovery offered by the preceding Facts.*

Composition of ammonia supposed to be ascertained.

Ammonia is a substance, the chemical composition of which has always been considered of late years as most perfectly ascertained, and the apparent conversion of it into hidrogen and nitrogen, in the experiments of Scheele, Priestley, and the more refined and accurate experiments of Berthollet, had left no doubt of its nature in the minds of the most enlightened chemists.

* Bakerian Lecture, 1806, page 8.

† In the year 1804, for a particular purpose of geological inquiry, I made an analysis of the porcelain clay of St. Stevens, in Cornwall, which results from the decomposition of the feldspar of fine-grained granite. I could not detect in it the smallest quantity of alkali. In making some experiments on specimens of the undecompounded rock taken from beneath the surface, there were evident indications of the presence of a fixed alkali, which seemed to be potash. So that it is very probable, that the decomposition depends on the operation of water and the carbonic acid of the atmosphere on the alkali forming a constituent part of the chrystalline matter of the feldspar, which may disintegrate from being deprived of it.

All

All new facts must be accompanied however by a train of analogies, and often by suspicions with regard to the accuracy of former conclusions. As the two fixed alkalis contain a small quantity of oxigen united to peculiar bases, may not the volatile alkali likewise contain it? was a query which soon occurred to me in the course of inquiry; and in perusing the accounts of the various experiments made on the subject, some of which I had carefully repeated, I saw no reason to consider the circumstance as impossible. For supposing hidrogen and nitrogen to exist in combination with oxigen in low proportion, this last principle might easily disappear in the analytical experiments of decomposition by heat and electricity, in water deposited upon the vessels employed or dissolved in the gasses produced.

But conjectured to contain oxigen.

Of the existence of oxigen in volatile alkali I soon satisfied myself. When charcoal carefully burnt and freed from moisture was ignited by the Voltaic battery of the power of 250 of 6 and 4 inches square, in a small quantity of very pure ammoniacal gas*: a great expansion of the aeriform matter took place, and a white substance formed, which collected on the sides of the glass tube employed in the process; and this matter, exposed to the action of diluted muriatic acid, effervesced, so that it was probably carbonate of ammonia.

This proved.

A process of another kind offered still more decisive results. In this the two mercurial gazometers of the invention of Mr. Pepys, described in No XIV of the Phil. Trans. for 1807 †, were used with the same apparatus, as that

A more decisive proof.

* The apparatus in which this experiment was made is described in page 214 Journal of the Royal Institution. The gas was confined by mercury, which had been previously boiled to expel any moisture that might adhere to it. The ammonia had been exposed to the action of dry pure potash, and a portion of it equal in volume to 10980 grains of mercury, when acted on by distilled water, left a residuum equal to 9 grains of mercury only. So that the gas, there is every reason to believe, contained no foreign aeriform matter; for even the minute residuum may be accounted for by supposing it derived from air dissolved in the water.

† See Journal, vol. XIX, p. 217.

employed

A more decisive proof.

employed by Mrs. Allen and Pepys for the combustion of the diamond, and these gentlemen kindly assisted in the experiment.

Very pure ammoniacal gas was passed over iron wire ignited in a platina tube, and two curved glass tubes were so arranged, as to be inserted into a freezing mixture; and through one of these tubes the gas entered into the platina tube, and through the other it passed from the platina tube into the airholder arranged for its reception.

The temperature of the atmosphere was 55°; and it was observed, that no sensible quantity of water was deposited in the cooled glass tube transmitting the unaltered ammonia, but in that receiving it after its exposure to heat moisture was very distinct, and the gas appeared in the airholder densely clouded.

This circumstance seems distinctly to prove the formation of water in this operation for the decomposition of ammonia; unless indeed it be asserted, that the hidrogen and nitrogen gasses evolved hold less water in solution or suspension than the ammonia decomposed, an idea strongly opposed by the conclusions of Mr. Dalton * and the experiments of Messrs. Desormes and Clement †.

After the gas had been passed several times through the ignited tube from one gazometer to the other, the results were examined. The iron wire became converted superficially into oxide, and had gained in weight $\frac{44}{100}$ parts of a grain, about $\frac{4}{10}$ of a grain of water were collected from the cooled glass tubes by means of filtrating paper, and 33·8 cubic inches of gas were expanded into 55·3 cubic inches, and by detonation with oxigen it was found, that the hidrogen gas in these was to the nitrogen as 3·2 to 1 in volume.

It will be useless to enter into the more minute details of this experiment, as no perfectly accurate data for proportions can be gained from them; for the whole of the ammonia was not decomposed, and as the gas had been prepared by being sent from a heated mixture of sal ammoniac and quicklime into the airholder, it was possible, that some solution of

* Manchester Memoirs, Vol. V, Part II, page 535, 1785.

† Annales de Chemie, Vol. XLII, p. 125.

ammonia

ammonia might have been deposited, which, by giving out new gas during the operation, would increase the absolute quantity of the material acted upon.

Berthollet's decomposition of ammonia by electricity. Products exceeded.

In examining the results of Mr. Berthollet's * elaborate experiments on the decomposition of ammonia by electricity, I was surprised to find, that the weight of the hidrogen and nitrogen produced rather exceeded than fell short of that of the ammonia considered as decomposed, which was evidently contradictory to the idea of its containing oxigen. This circumstance, as well as the want of coincidence between the results and those of Priestley and Van Marum on the same subject, induced me to repeat the process of electrization of ammonia, and I soon found, that the quantities of the products in their relations to the apparent quantity of gas destroyed were influenced by many different causes.

Quantities of the products influenced by various causes.

Ammonia procured over dry mercury from a mixture of dry lime and muriate of ammonia, I found, deposited moisture upon the sides of the vessel, in which it was collected, and in passing the gas into the tube for electrization, it was not easy to avoid introducing some of this moisture, which must have been a saturated solution of ammonia, at the same time.

In my first trials, made upon gas passed immediately from the vessel in which it had been collected into the apparatus, I found the expansion of 1 of ammonia vary in different instances from 2·8 to 2·2 measures, but the proportions of the nitrogen and hidrogen appeared uniform, as determined by detonation of the mixed gas with oxigen, and nearly as 1 to 3 in volume.

To exclude free moisture entirely, I carefully prepared ammonia in a mercurial airholder, and after it had been some hours at rest, passed a quantity of it into the tube for decomposition, which had been filled with dry mercury. In this case 50 parts became 103 parts by electrization, and there was still reason to suspect sources of errour.

I had used iron wires not perfectly free from rust for taking the spark, and a black film from the mercury appeared on the sides of the tube. It was probable, that some ammonia had been absorbed by the metallic oxides both upon the

* *Mémoires de l'Académie*, 1785, page 324.

iron

iron and the mercury, which might again have been given out in the progress of the operation.

I now used recently distilled mercury, which did not leave the slightest film on the glass tube, and wires of platina. The ammonia had been exposed to dry caustic potash, and proved to be equally pure with that mentioned in page 326. 60 measures of it, each equal to a grain of water, were electrized till no farther expansion could be produced, the gas filled a space equal to that occupied by 108 grains of water. The thermometer in this experiment was at 56°, and the barometer at 30·1 inches. The wire of platina transmitting the spark was slightly tarnished *. The 108 measures of gas, carefully analyzed, were found to consist of 80 measures in volume of hidrogen, and 28 measures of nitrogen.

Specific gravity of ammonia.

The results of an experiment that I made in 1799 † give the weight of 100 cubic inches of ammonia as 18·18 grains at the mean temperature and pressure. I had reasons however for suspecting, that this estimation might be somewhat too low, and on mentioning the circumstance to Messrs. Allen and Pepys, they kindly undertook the examination of the subject, and Mr. Allen soon furnished me with the following data. "In the first experiment 21 cubic inches of ammonia weighed 4·05 grains; in a second experiment the same quantity weighed 4·06 grains, barometer 30·65, thermometer 54° Fahrenheit."

Now if the correctness for temperature and pressure be made for these estimations, and a mean taken, 100 cubic inches of ammonia will weigh 18·67 grains, barometer being at 30, and thermometer at 60° Fahrenheit: and if the quantity used in the experiment of decomposition be calculated upon as cubic inches, 60 will weigh 11·2 grains. But the hidrogen gas evolved equal to 80 will weigh 1·93 ‡ grains, and the nitrogen equal to 28 §, 8·3. And 1·9+ 8·3

* This most probably was owing to oxidation. When platina is made positive in the Voltaic circuit in contact with solution of ammonia, it is rapidly corroded. This is an analogous instance.

† Researches Chem. and Phil. p. 62.

‡ Lavoisier's Elements, p. 569. A cubical inch of hidrogen is considered as weighing ·0239.

§ Researches Chem. and Phil. page 9. From my experiments 100

8·3=10·2; and 11·2−10·2=1; all the estimations being made according to the standard temperature and pressure.

Products only $\frac{10}{11}$, therefore $\frac{1}{11}$ oxigen.

So that in this experiment on the decomposition of ammonia, the weight of the gasses evolved is less by nearly $\frac{1}{11}$ than that of the ammonia employed; and this loss can only be ascribed to the existence of oxigen in the alkali; part of which probably combined with the platina wires employed for electrization, and part with hidrogen.

Ammonia probably contains more than ·08.

After these ideas the oxigen in ammonia cannot well be estimated at less than 7 or 8 parts in the hundred; and it possibly exists in a larger proportion, as the gasses evolved may contain more water than the gas decomposed, which of course would increase their volume and their absolute weight *.

Supposing it a triple compound, the phenomena easily accounted for.

In supposing ammonia a triple compound of nitrogen, hidrogen, and oxigen, it is no less easy to give a rational account of the phænomena of its production and decomposition, than in adopting the generally received hypothesis of its composition.

Oxigen, hidrogen, and nitrogen are always present in cases in which volatile alkali is formed; and it usually appears during the decomposition of bodies in which oxigen is loosely attached, as in that of the compounds of oxigen and nitrogen dissolved in water.

Ammonia analogous to the acids with compound bases.

At common temperatures under such favourable circumstances, the three elements may be conceived capable of combining, and of remaining in union: but at the heat of ignition the affinity of hidrogen for oxigen prevails over the complex attraction, water is formed, and hidrogen and nitrogen are evolved; and according to these conclusions, ammonia will bear the same relations to the fixed alkalis, as the vegetable acids with compound bases do to the mineral ones with simple bases.

100 cubical inches of nitrogen weigh, at the standard temperature and pressure, 29·6 grains.

Quantity of water in gasses to be known only by electrization.

* In the present state of our knowledge, perfectly correct data for proportions cannot probably be gained in any experiments on the decomposition of ammonia, as it seems impossible to ascertain the absolute quantity of water in this gas; for electrization, according to Dr. Henry's ingenious researches, offers the only means known of ascertaining the quantity of water in gasses.

Oxigen the principle of alkalinity.

Oxigen then may be considered as existing in, and as forming an element in all the true alkalis; and the principle of acidity of the French nomenclature might now likewise be called the principle of alkalescence.

The alkaline earths probably oxidized metals.

From analogy alone it is reasonable to expect, that the alkaline earths are compounds of a similar nature to the fixed alkalis, peculiar highly combustible metallic bases united to oxigen. I have tried some experiments upon barytes and strontities; and they go far towards proving, that this must be the case. When barytes and strontites, moistened with water, were acted upon by the power of the battery of 250 of 4 and 6, there was a vivid action and a brilliant light at both points of communication, and an inflammation at the negative point.

Barytes and strontia appear to be so.

In these cases the water might possibly have interfered. Other experiments gave however more distinct results.

Barytes and strontites, even when heated to intense whiteness in the electrical circuit by a flame supported by oxigen gas, are nonconductors; but by means of combination with a very small quantity of boracic acid, they become conductors; and in this case inflammable matter, which burns with a deep red light in each instance, is produced from them at the negative surface. The high temperature has prevented the success of attempts to collect this substance; but there is much reason to believe, that it is the bases of the alkaline earth employed.

Inflammable matter produced from them.

Barytes and strontites have the strongest relations to the fixed alkalis of any of the earthy bodies*; but there is a chain of resemblances, through lime, magnesia, glucina, alumina, and silex. And by the agencies of batteries sufficiently strong, and by the application of proper circum-

Probably other earths may be analyzed by electricity.

Earths long ago considered analogous to metallic oxides.

* The similiarity between the properties of earths and metallic oxides was noticed in the early periods of chemistry. The poisonous nature of barytes, and the great specific gravity of this substance as well as of strontites, led Lavoisier to the conjecture, that they were of a metallic nature. That metals existed in the fixed alkalis seems however never to have been suspected. From their analogy to ammonia, nitrogen and hidrogen have been supposed to be amongst their elements. It is singular, with regard to this class of bodies, that those most unlike metallic oxides are the first which have been demonstrated to be such.

stances

stances, there is no small reason to hope, that even these refractory bodies will yield their elements to the methods of analysis by electrical attraction and repulsion.

In the electrical circuit we have a regular series of powers of decomposition, from an intensity of action, so feeble as scarcely to destroy the weakest affinity existing between the parts of a saline neutral compound, to one sufficiently energetic to separate elements in the strongest degree of union in bodies undecomposable under other circumstances. Powers of electricity form a regular series.

When the powers are feeble, acids and alkalis, and acids and metallic oxides, merely separate from each other; when they are increased to a certain degree, the common metallic oxides and the compound acids are decomposed; and by means still more exalted, the alkalis yield their elements. Their action. And as far as our knowledge of the composition of bodies extends, all substances attracted by positive electricity are oxigen, or such as contain oxigen in excess; and all that are attracted by negative electricity are pure combustibles, or such as consist chiefly of combustible matter. Oxigen attracted by positive electricity, combustible matter by negative.

The idea of muriatic acid, fluoric acid, and boracic acid containing oxigen, is highly strengthened by these facts. And the general principle confirms the conjecture just stated concerning the nature of the earths.

In the electrization of boracic acid moistened with water, I find, that a dark coloured combustible matter is evolved at the negative surface; but the researches upon the alkalis have prevented me from pursuing this fact, which seems however to indicate a decomposition. Boracic acid.

Muriatic acid and fluoric acid in their gaseous states are nonconductors: and as there is every reason to believe, that their bases have a stronger attraction for oxigen than water, there can be little hope of decomposing them in their aqueous solutions, even by the highest powers. In the electrization of some of their combinations there is however a probability of success. Muriatic and fluoric acid.

An immense variety of objects of research is presented in the powers and affinities of the new metals produced from the alkalis. New metals afford a large field of research.

In

An instrument of decomposition.

In themselves they will undoubtedly prove powerful agents for analysis; and having an affinity for oxigen stronger than any other known substances, they may possibly supersede the application of electricity to some of the undecompounded bodies.

Base of potash decomposes carbonic acid.

The bases of potash I find oxidates in carbonic acid and decomposes it, and produces charcoal when heated in contact with carbonate of lime. It likewise oxidates in muriatic acid; but I have had no opportunity of making the experiment with sufficient precision to ascertain the results.

Geology.

In sciences kindred to chemistry, the knowledge of the nature of the alkalis, and the analogies arising in consequence, will open many new views; they may lead to the solution of many problems in geology, and show, that agents may have operated in the formation of rocks and earths, which have not hitherto been suspected to exist.

It would be easy to pursue the speculative part of this inquiry to a great extent, but I shall refrain from so occupying the time of the Society, as the tenour of my object in this lecture has not been to state hypotheses, but to bring forward a new series of facts.

II.

On the Composition of the Compound Sulphuret from Huel Boys, and an Account of its Crystals. *By* James Smithson, *Esq. F. R. S.**.

Compound sulphuret from Huel Boys.

IT is but very lately, that I have seen the Philosophical Transactions for 1804, and become acquainted with the two papers on the compound sulphuret of lead, antimony, and copper contained in the first part of it †; which circumstance has prevented my offering sooner a few observations on Mr. Hatchett's experiments, which I deem essential towards this substance being rightly considered, and indeed the principles of which extend to other chemical compounds; and also giving an account of this compound sulphuret,

* Philos. Trans. for 1807, Part 1, p. 55.

† See Journal, vol. IX, p. 14.

phuret, as that which had been laid before the Society is very materially inaccurate and imperfect.

We have no real knowledge of the nature of a compound substance, till we are acquainted with its proximate elements, or those matters by the direct or immediate union of which it is produced; for these only are its true elements. Thus, though we know that vegetable acids consist of oxigen, hidrogen, and carbon, we are not really acquainted with their composition, because these are not their proximate, that is, are not their elements, but are the elements of their elements, or the elements of these. It is evident what would be our acquaintance with sulphate of iron, for example, did we only know that a crystal of it consisted of iron, sulphur, oxigen, and hidrogen; or of carbonate of lime, if only that it was a compound of lime, carbon or diamond, and oxigen. In fact, totally dissimilar substances may have the same ultimate elements, and even probably in precisely the same proportions; nitrate of ammonia, and hydrate of ammonia, or crystals of caustic volatile alkali*, both ultimately consist of oxigen, hydrogen, and azote.

To know the nature of a compound we must find its proximate elements.

It is not probable, that the present ore is a direct quadruple combination of the three metals and sulphur, and that these, in their simple states, are its immediate component parts; it is much more credible, that it is a combination of the three sulphurets of these metals.

The ore probably composed of three sulphurets.

On this presumption I have made experiments to determine the respective proportions of these sulphurets in it.

I have found 10 grains of galena, or sulphuret of lead, to produce 12·5 grains of sulphate of lead. Hence the 60·1 grains of sulphate of lead, which Mr. Hatchett obtained, correspond to 48·08 grains of sulphuret of lead.

10 grs sulphuret of lead produce 12·5 sulphate.

I have found 10 grains of sulphuret of antimony to afford 11 grains of precipitate from muriatic acid by water. Hence 31·5 grains of this precipitate are equal to 28·64 grains of sulphuret of antimony.

10 grs sulphuret of antimony 11 grs sulphate.

The want of sulphuret of copper has prevented my determining the relation between it and black oxide of copper,

* Fourcroy, *Syst. des Con. Chem. t.* I. p. lxxxviii. Transl. 1, 100.

but

multitude of instances which I could adduce, in support of such being the fact, I will for the sake of brevity confine myself to a few, in the substances which have come under consideration above, as they will likewise give the grounds, on which some of the proportions in the table have been assigned, and every chemist, by a careful repetition of the experiments, may easily determine for himself to what attention the present theory is entitled.

Instances.

Lead - -	$= \frac{3}{2}$ of sulphate of lead
	$= \frac{6}{5}$ of sulphuret of lead
Sulphuret of lead	$= \frac{5}{6}$ of lead
	$= \frac{5}{4}$ of sulphate of lead
Sulphate of lead	$= \frac{2}{3}$ of lead
	$= \frac{4}{5}$ of sulphuret of lead
Antimony -	$= \frac{4}{3}$ of powder of algaroth
	$= \frac{6}{5}$ of sulphuret of antimony
Sulphuret of antimony -	$= \frac{10}{9}$ of powder of algaroth.

In the experiments by which these relations were ascertained, the portion of powder of algaroth and sulphate of lead dissolved in the precipitating and washing waters was scrupulously collected.

Perhaps the quantity of an element expresses its force of attraction.

The importance of a knowledge of the true quantity in which matters combine is too evident, to require to be dwelt upon; but this importance will be greatly augmented, if it should prove, that this quantity is, as has been suggested, expressive of the forces with which they attract each other. It is perhaps in the form of matters, that we shall find the cause of the proportions in which they unite, and a proof, *a priori*, of the system here maintained.

Gray copper ore.

I have examined some of the gray ores of copper in tetraedral crystals; but the notes of my experiments are in England. I can however, say, that they do contain antimony, and that they do not contain iron in any material quantity. With respect to the proportions of the constituent parts, I cannot now speak with any certainty; but, I think, that at least some species of fahlertz contain a smaller portion of sulphuret of antimony, than the fahlertz does which exists as an element in the foregoing compound one.

Of

Of the Form of this Substance.

Of the seventeen figures which have been given, as of the crystals of this compound sulphuret, in Part II of the volume of the Transactions for 1804, great part are acknowledged to have no existence, nor are indeed any of them consistent with nature.

Form of the compound sulphuret.

This substance seems to have yet offered but one form, which is represented in Plate 9 under its two principal appearances; that is, having the primitive faces the predominant ones of the prism; and having the secondary ones such, and which will be fully sufficient to make it known. In the first infancy of the study of crystals, it might be necessary to attend to every, the most trifling, variation of them, to trace each of their changes step by step, to spell as it were, the subject; but in the state to which the science has now attained, to continue to do so would be not only superfluous, but most truly puerile.

I have a very small, but very regular, crystal of the form of Fig. 1.

By mensuration the faces *a* and *m* appear to form together an angle of about 135°, and the faces *c* and *b* an angle of about 125°.

It is said in the account above quoted, that the primitive form of this matter is a rectangular tetraedral prism, but no proofs of this have been offered; nor have the dimensions of this prism been given, a circumstance of the first moment to the determination of true or primitive form, nor have any quantities been assigned to the decrements supposed. I will, therefore, supply these very important omissions.

Dimensions of the prism necessary to determine the primitive form.

That the atom of this substance is a rectangular tetraedral prism, is inferable, not from the striæ on the crystals, for striæ are by no means invariably indicative of a decrement in the direction of them; but from the angles which the faces *a* and *c* make with the faces *m* and *b*; and these angles also prove, that the height of this prism is equal to the side of its base, that is, that it is a cube.

It is a cube.

Hence the face *a* is produced by a decrease of one row of atoms along the edge of the cube, and the angle it forms with the face *m* is really of 135°.

Its decrements.

The face c is produced by a decrease of two rows of atoms at the corners of the cube, and the angle it forms with the face b is $= 125° 15' 52''$.

The face b being produced like the face a, forms the same angle with the face m.

No crystal I possess has enabled me to measure the inclinations of the faces g, d, or f; should the face g, as is presumable, result from a decrease of one row of atoms at the corners of the cube, it will form with the face b an angle of $144° 44' 8''$; and if the faces d and f are, as is also probable, produced by a decrease of two rows of atoms along the edges of the cube, the first will form an angle of $116° 33' 54''$, and the latter one of $153° 26' 6''$, with the face m.

This differs from the former account of the crystals.

The angles assigned here differ considerably from those given in the former account of these crystals; but the angles there given have not only appeared to me to be contradicted by observation, but, crystallographically considered, are inconsistent with each other, as the tetraedral prism of dimensions to produce an angle of 135° by a decrement along its edge would not afford angles of 140° and 120° by decrements at its corners.

The sum of the faces of these crystals is 50.

III.

On a new Property of the Tangents of the three Angles of a Plane Triangle. By Mr. WILLIAM GARRARD, *Quarter Master of Instruction at the Royal Naval Asylum at Greenwich. Communicated by the Astronomer Royal*.*

Sum of three tangents of a plane triangle multiplied by square of radius equal to their continued product. Demonstrated in an acute angled triangle:

PROPOSITION I. In every acute angled plane triangle, the sum of the three tangents of the three angles multiplied by the square of the radius is equal to the continued product of the tangents.

Demonstration.—Let AH, HI, and IB, Plate 9 Fig. 3, be the arches to represent the given angles; and AG, HK, and BT be their tangents, put r the radius, $AG=a$, and $BT=b$,

* Philos. Trans. for 1807, Part I, p. 120.

Then

Then $\frac{r^2}{a}$ and $\frac{r^2}{b}$ will be the tangents of HD and DI.

Now by Prop. VIII, Sect. I, Book I, Emerson's Trigonometry,

As radius square—product of two tangents
Is to radius square,
So is the sum of the tangents
To the tangent of their sum.

$$\therefore r^2 - \frac{r^4}{ab} : r^2 :: \frac{r^2}{a} + \frac{r^2}{b} : \frac{r^2a + r^2b}{ab - r^2} = \text{HK};$$

therefore $a + b + \frac{r^2a + r^2b}{ab - r^2} = \frac{a^2b + ab^2}{ab - r^2} =$ the sum of the three tangents,

and $\frac{a^2b + ab^2}{ab - r^2} \times r^2 = ab \times \frac{r^2a + r^2b}{ab - r^2} =$ their continued product. Q. E. D.

Proposition II. In every obtuse angled plane triangle, the sum of the three tangents of the three angles multiplied by the square of the radius is equal to their continued product.

Demonstration.—Let AH, Fig. 4, be an obtuse arc, and HE, EB the other two. and in an obtuse angled triangle.

Then BF, ED, and AG are the three tangents.

Put BF $= t$ and DE $= u$ radius $= r$, then per trigonometry, as before, $r^2 \times \frac{t + u}{r^2 - tu} =$ BT;

But $-$ BT $=$ AG $= - \frac{t + u}{r^2 - tu} \times r^2$.

Wherefore $t + u - \frac{t + u}{r^2 - tu} \times r^2 =$ the sum of the three tangents, which being reduced

is $= - tu \times \frac{t + u}{r^2 - tu}$, and multiplied into r^2 is equal to

$tu \times - \frac{t + u}{r^2 - tu} \times r^2 =$ the product. Q. E. D.

IV.

On a new Property of the Tangents of three Arches trisecting the Circumference of a Circle. By NEVIL MASKELYNE, *D. D. F. R. S. and Astronomer Royal*.*

Sum of three tangents of three arches trisecting a circle, multiplied by radius, equal to their product

MR. William Garrard having shown me a curious property of the tangents of the three angles of a plane triangle, or in other words, of the tangents of three arches trisecting a semicircle, in a paper which I have communicated to this Society, I was led to consider, whether a similar property might not belong to the tangents of three arches trisecting the whole circumference; and, on examination, found it to be so.

Let the circumference of a circle be divided any how into three arches A, B, C; that is, let $A + B + C$ be equal to the whole circumference. I say, the square of the radius multiplied into the sum of the tangents of the three arches A, B, C, is equal to the product of the tangents multiplied together. I shall demonstrate this by symbolical calculation, now commonly called (especially by foreign mathematicians) analytic calculation.

Preliminary remark.

It may be proper to premise, that the signification of the symbolical expressions of the tangents of an arc, whether with respect to geometry or numbers, are to be understood according to their position as lying on one side, or the other side of the radius, passing through the point of commencement of the arc of the circle; those tangents which belong to the first or third quadrant of the circle being considered as positive, and those belonging to the second and fourth quadrant, being of a contrary direction, as negative; in like manner as the sines in the first semicircle are considered as positive, and in the second semicircle as negative; and the cosines in the first and fourth quadrant are considered as positive, and in the second and third quadrants as negative; they lying, in the second case, on the contrary side of the diameter passing through the point of ninety degrees, to what they do in the former. Hence it easily follows, that the tan-

* Ibid. p. 122.

gent

gent of any arch and of its supplement to the whole circumference, or 360 degrees, are equal and contrary to one another, or the one negative of the other.

Let t, u, w, be put for the tangents of the three arches A, B, C respectively, and r for the radius, and $\odot$ for the whole circumference. Then $A+B+C=\odot$, and $C=\odot-\overline{A+B}$. Demonstration.

By trigonometry, $t, \overline{A+B}=\frac{r^2\times\overline{t+u}}{r^2-tu}$, and the tang. C = tang. $(\odot-\overline{A+B})=-$ tang. $\overline{A+B}$, by what has been said above. Therefore $t, A+t, B+t, C$ or $t+u+w=t+u-\frac{r^2\times\overline{t+u}}{r^2-tu}$ $=tu\times-\frac{r^2\times\overline{t+u}}{r^2-tu}$; but t and u are the expressions for the tangents of A and B respectively, and $-\frac{r^2\times\overline{t+u}}{r^2-tu}$ is the expression for the tangent of C, or for w. Therefore, $r^2\times\overline{t+u+w}$, or the square of the radius multiplied into the sum of the three tangents of A, B, and $C=tuw$, or the product of the tangents. Q. E. D.

V.

On the apparent Radiation and Reflection of Cold by means of two concave metallic Mirrors. In a Letter from Mr. JOHN MARTIN.

To Mr. NICHOLSON,

SIR,

THERE are many phenomena, exhibited to the notice of the chemical philosopher in the course of his arduous research, that are not so well understood as perhaps the present state of science might lead him to expect. Some of these phenomena have hitherto been totally inexplicable; others have not been explained with all the clearness and perspicuity that could be wished. Among the number of the latter may be ranked the apparent radiation and reflection of cold by means of two concave metallic mirrors.

Some chemical facts not sufficiently explained.

Apparent radiation and reflection of cold.

This

This curious fact, notwithstanding we are so well acquainted with the laws that govern heat during its passage through and impingency upon bodies, has never, I believe, been illustrated with sufficient clearness.

The cold body supposed to receive heat from the thermometer.

The explanations that have hitherto been given rest principally for support on the supposition, that the thermometer placed in the focus of one mirror acts as a heated body, and that the heat radiating from it is transmitted to the cold body in the opposite focus. The thermometer, however, is in fact not a heated body, since it is not hotter than the surrounding atmosphere, and consequently cannot radiate caloric: but it is said, the surrounding air becomes cooled, and consequently the thermometer in respect to it is a hot body, and radiates caloric accordingly. This however does not explain clearly why the thermometer should be reduced to a temperature lower than the air which surrounds it, which will be found to be the case; or at least, it leaves too much to be supplied by the imagination. I trust I shall be able to render this matter clearer.

But this cannot give out radiant heat.

Another mode of accounting for it.

There are only two ways, in which heat can be made to move in one direction through any given body, we will suppose a wire A $\underline{\;x\;\;y\;\;z\;}$ B; one is the application of a superior temperature to B, causing the heat to move on towards A by the conducting power of the wire, and the tendency of the caloric to establish an equilibrium; the other is, to reduce the temperature at A, and thus cause a partial vacuity of heat, which must of necessity be filled up by a fresh quantity from toward x, which will receive again a fresh supply from toward y, and that from towards z, &c., and by this means induce a current of heat from B to A, till an equilibrium is established. It is upon this principle, the filling up of partial vacuities of heat (if I may be allowed the expression), that the rational explanation of the phenomenon in question can be grounded. Fact puts this sufficiently beyond a doubt, and it now remains to show how it is effected.

How this is effected.

It will scarcely be necessary to mention in this place, that, when a particle of heat impinges upon a plane reflecting surface, it is thrown off in an angle equal to that with

with which it is thrown upon it. Now, on the contrary, if a cold body, *b*, Fig. 5, Pl. 9, be brought near a plane reflecting surface, as particles of heat are entering into that body in all directions from the surrounding air, some particles of heat must be entering into it in the direction *a b*, consequently the point *a* of the reflecting surface must become cooler, or, to use my former expression, a vacuity of heat will be there formed: now it may be demonstrated, that this vacuity or space will not be supplied by heat moving in the direction *x a*, *y a*, or *z a*, but will be supplied by heat moving in no other direction than *c a*, which heat, striking against the point *a*, will be thrown off into the body *b*; the angle *c a d* being equal to the angle *b a e*, and bodies will move in the direction in which they meet with the least resistance; for if heat were to come from any other direction but *c a*, it would not be reflected towards the body *b*, but elsewhere, and consequently, to join the current of heat *a b*, it must again change its course. Hence it follows, that, when a cold body is brought near a plane reflecting surface, in proportion as the surrounding air becomes cool, heat will enter into that body in right lines tending to its centre; the plane reflecting surface will have its temperature lowered, and particles of heat will strike upon every part of it in such directions, as to be thrown off in right lines to the cool body.

The application of this fact to the explanation of the phenomenon in question will be readily perceived, substituting concave reflecting surfaces instead of plane ones: the heat enters into the cold body placed in the focus of one mirror (B. fig. 6) from the surrounding air in all directions, consequently every point of the surface of the mirror, *a*, *b*, *c*, *d*, &c., becomes cooled, and those points can only receive a fresh supply in parallel rays, in a direct course from the opposite mirror, because only such rays (striking against so many imaginary tangents *a*, *b*, *c*, *d*, of that mirror) can be thrown off towards the body B; the opposite mirror therefore becomes cool, and for the same reason the whole surface of it must be supplied by heat from the thermometer T, which consequently must become cooler than a body placed any where in its neighbourhood.

This applied to a concave surface.

If

If you think proper, an insertion of this explanation in your valuable Journal will greatly oblige,

Sir,

Your most obedient Servant,

Old Broad Street, JOHN MARTIN.
19 *July*, 1808.

VI.

Description of a Balance Level, useful for laying out Land for Irrigation, for Roads, and other Purposes. By Mr. RICHARD DREW, *of Great Ormond Street* *.

SIR,

Balance level useful in draining and watering land.

HEREWITH you will receive a Balance Level, of my invention, which I have satisfactorily used on several gentlemen's estates in Devonshire, where I have been employed to drain and carry water to irrigate meadow land. I have made several for persons in that county, whose employment is to drain and irrigate land, and they have found it to answer their purpose better than the spirit or water level, it being more portable and ready to the sight.

I have lately used it on Mr. Satterley's farm, at Hastings, to carry the water of his closes over several acres of dry ground. Dr. De Salis, who has seen it, advised me to send it to the Society of Arts, &c., that they might judge of its merits.

I am, Sir,

Your obedient Servant,

RICHARD DREW.

Explanation of the Method of using the Instrument.

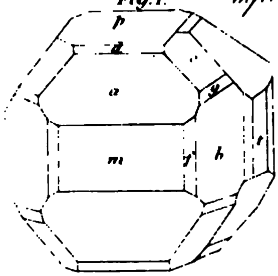
Method of using it.

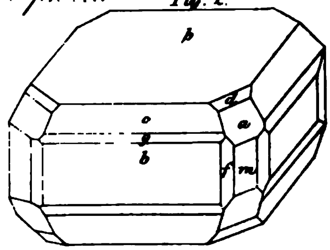
Set it on a triangular staff, and point it at the object staff, which is held by another person at a distance; move the level on the joint, until the inner tube plays clear within

* Trans. of the Society of Arts for 1807, p. 22. The Society voted Mr. Drew ten guineas for this invention.

the

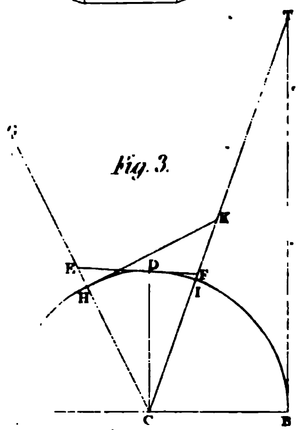
Fig. 1. Compound Sulphurets Fig. 2.





New Properties of Tangents

Fig. 3.



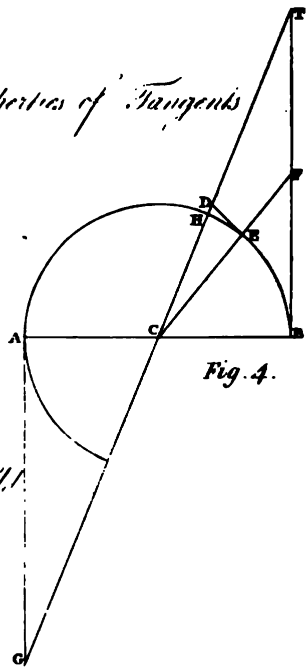


Fig. 4.

Radiation & Reflection of Light

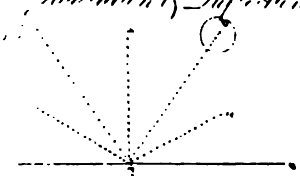
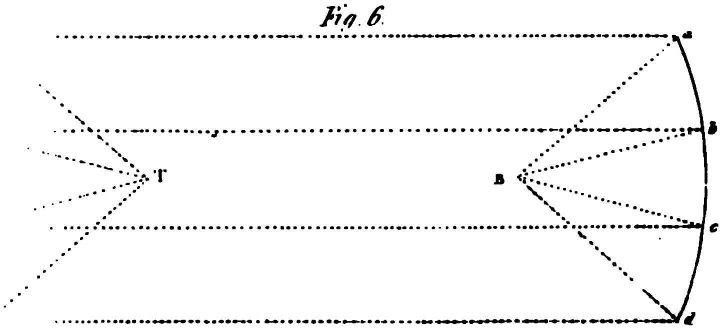


Fig. 6.



the outer tube. Look through the sights, and observe the object staff which the person holds, let him move the slide on the staff, until you see the hair cut the middle of the slide, on which there is a black line; then turn the level round, look through the sights, and see if the hair cuts the middle of the slide as before; if it does, it will be level; but if there be a difference in both ends, the person who holds the staff must set the slide to half that difference. You are then to adjust the level by turning with a key the screw, which moves the balance contained in the bottom of the inner tube.

Is used with quickness and accuracy.

Certificates from Mr. J. W. Gooch, Mr. Charles Layton, and Mr. Benj. Holmes, testify, that they have seen in use the level invented by Mr. Richard Drew, and that the business is done by it with accuracy and dispatch.

Reference to the Engraving of Mr. Richard Drew's Balance Level. Plate X, *Fig.* 1, 2, 3, 4.

The instrument described.

Fig. 1. The balance level, mounted on a ball and socket joint, with a tube, *a*, to fix on a stand.

Fig. 2. A section, *b b c c* two tubes of tin, which slide on a short tube, *d d*, placed in the middle, and having an iron wire soldered round it to stiffen it, and to serve as a shoulder,

e e Two eye pieces, with glass in both, one at each end, and sliding into the tubes *b* and *c*.

f f The balance level, hanging by a sort of staple *g*, on a point fixed upright on the middle of the bar *h* (shown in Fig. 3), which is fastened across the tube *d*.

i i Two eye pieces sliding into the ends of the level *f f*, and having a narrow slit horizontally across the middle, with a hair before each, shown by the dots *h h*.

k An adjusting screw, which acts by drawing the piece *m*, (which moves in a dove-tail slide), in one end of the tube.

n The key-hole through which the screw is turned.

Fig. 4. An end view of the case and level, showing the eye pieces *i* and *e*, one within the other.

VII. *Account*

VII.

Account of a new Method of rearing Poultry to Advantage. By Mrs. HANNAH D'OYLEY, *of Sion Hill, near North-allerton, Yorkshire*.*

SIR,

Cheap and easy method of rearing poultry.

I BEG leave to communicate a most desirable method of rearing poultry, which I have proved by experience; the economy and facility, with which it may be performed, would, if generally adopted, lower the price of butchers' meat, and thereby be of essential benefit to the community at large. I keep a large stock of poultry, which are regularly fed in a morning upon steamed potatoes chopped small, and at noon they have barley; they are in high condition, tractable, and lay a very great quantity of eggs. In the poultry yard is a small building, similar to a pigeon cote, for the hens to lay in, with frames covered with net to slide before each nest; the house is dry, light, and well ventilated, kept free from dirt by having the nests and walls white-washed two or three times a year, and the floor covered once a week with fresh ashes. When I wish to procure chickens, I take the opportunity of setting many hens together, confining each to her respective nest; a boy attends morning and evening to let any off that appear restless, and to see that they return to their proper places. When they hatch, the chickens are taken away, and a second lot of eggs allowed them to set again, by which means they produce as numerous a brood as before: I put the chickens into long wicker cages, placed against a hot wall at the back of the kitchen fire, and within them have artificial mothers for the chickens to run under; they are made similar to those described by Monsieur Reaumur, in "his *Art de faire éclore et d'élever en toutes Saisons des Oiseaux domestiques de toutes Espèces,*" &c., in two volumes, printed at Paris, 1751. They are made of boards about ten inches

Food.

Poultry house.

Breeding.

Rearing chickens.

Artificial mother.

* Trans. of the Society of Arts for 1807, p. 24. The silver medal was voted to Mrs. D'Oyley.

broad,

broad, and fifteen inches long, supported by two feet in the front, four inches in height, and by a board at the back two inches in height. The roof and back are lined with lamb's skins dressed with the wool upon them. The roof is thickly perforated with holes for the heated air to escape; they are formed without bottoms, and have a flannel curtain in front and at the ends for the chickens to run under, which they do apparently by instinct. The cages are kept perfectly dry and clean with sand or moss. The above is a proper size for fifty or sixty new hatched chickens, but as they increase in size, they of course require a larger mother. When they are a week old, and the weather fine, the boy carries them and their artificial mother to the grass-plot, nourishes and keeps them warm, by placing a long narrow tin vessel filled with hot water at the back of the mother, which will retain its heat for three hours, and is then renewed fresh from the steamer. In the evening they are driven into their cages, and resume their station at the hot wall, till they are nearly three weeks old, and able to go into a small room, appropriated to that purpose. The room is furnished with frames similar to the artificial mothers, placed round the floor, and with perches conveniently arranged for them to roost upon.

Cages.

Airing.

When I first attempted to bring up poultry in the above way, I lost immense numbers by too great heat and suffocation, owing to the roofs of the mothers not being sufficiently ventilated, and when that evil was remedied, I had another serious one to encounter; I found chickens brought up in this way did not thrive upon the food I gave them, and many of them died, till I thought of getting coarse barley-meal, and steaming it till quite soft. The boy feeds them with this and minced potatoes alternately; he is also employed rolling up pellets of dough, made of coarse wheat flour, which he throws to the chickens to excite them to eat, thereby causing them to grow surprisingly.

Numbers lost by too great heat and closeness.

Food.

I was making the above experiments in the summer for about two months, and during that time my hens produced me upwards of five hundred chickens, four hundred of which I reared fit for the table or market. I used a great many made into pies for the family, and found them cheaper than

In two months 400 reared.

butchers'

Might be sold with much profit as cheap as butchers' meat.

butchers' meat. Were I situated in the neighbourhood of London, or any very populous place, I am confident I could make an immense profit, by rearing different kinds of poultry in the above method for the markets, and selling them on an average at the price of butcher's meat.

A child might bring up some thousands in a season.

A young person of twelve or fourteen years of age might bring up in a season some thousands, and by adopting a fence similar to the improved sheep-fold, almost any number might be cheaply reared, and with little trouble. Hens kept as mine are, and having the same conveniences, will readily set four times in a season, and by setting twice each time, they would produce at the lowest calculation, eighty chickens each, which would soon make them very plentiful.

One hen might produce 80 chickens a year.

If this information should be so fortunate as to merit the approbation of the Society, I shall consider myself highly honoured, and my time as having been usefully employed.

I am, Sir,

Your most obedient Servant,

HANNAH D'OYLEY.

Farther account of the mode of managing them.

The most convenient size of an artificial mother for forty or fifty young chickens is about fifteen inches long, ten deep, four high in front, and two at the back; it is placed in a long wicker cage against a warm wall, the heat at about eighty degrees of Fahrenheit's thermometer, till the chickens are a few days old, and used to the comfort of it, after which time they run under when they want rest, and acquire warmth by crowding together. I find it advisable, to have two or three chickens among them of about a week old, to teach them to peck and eat. The meat and water is given them in small troughs fixed to the outside of the cage, and a little is strewed along from the artificial mother, as a train to the main deposit. It would have given me great pleasure, to have been able to send a specimen of my superior feed and management, if the season had been rather more advanced, for I think it is not possible for turkies and chickens to weigh heavier, to be whiter, or altogether better fed than mine are.

After

After a certain age, they are allowed their liberty, living chiefly on steamed potatoes, and being situated tolerably secure from the depredations of men and foxes, are permitted to roost in trees near the house.

According to your request, I herewith send you a rough sketch of the apparatus I use, which probably will convey an idea of the business, and not be too complicated for persons employed in poultry yards, fully to understand; but to prevent trouble and prejudice in the first onset, I think it necessary to remark, that if the chickens do not readily run under the artificial mother for want of some educated ones to teach them, it will be proper to have the curtain in front made of rabbit or hare skin, with the fur side outwards, for the warmth and comfort to attract them, afterwards they run under the flannel ones, which are preferable for common use, on account of cleanliness, and not being liable to get into the mouths of the chickens. Apparatus

I have had great amusement in rearing poultry in the above way, and if my time was not occupied with my children and other family concerns, I should most assuredly farm very largely in poultry.

Reference to the Engravings of Mrs. D'Oyley's Method of breeding Poultry, Plate X, *Fig.* 5, 6, 7.

Fig. 5. The apparatus called the artificial mother, with a curtain of green baize in front and at each end, and holes through the top to allow the circulation of air. described.

Fig. 6. Another view of the artificial mother, but without the curtain, in order to show its sloping direction, and interior lining of woolly sheep-skin.

Fig. 7. A wicker basket four feet long, two feet broad, and fourteen inches high, with a lid to open, and a wooden sliding bottom similar to a bird cage: the artificial mother is shown, as placed within it.

O. A trough in front to hold food for the chickens.

Remark.

As the cheapness with which fowls can be reared in this way is an object of primary consideration, it is to be regretted, that Mrs. D'Oyley has not added an account of the

the quantity of food consumed by a certain number of chickens in a given time; as on this must depend the price at which they could be sold, and the profit that might be made of them. This would have been attended with another advantage, it would have been a guide with respect to the quantity of the different kinds of food, with which the chickens ought to be supplied in the several stages of their growth, to those who have not been in the habit of rearing poultry; and this must necessarily be the case with many persons in the vicinity of London in particular, to whom the adoption of Mrs. D'Oyley's plan might be very desirable. Mrs. D'Oyley does not say whether the turkeys she mentions were reared in the same way.

VIII.

Communication from the Right Hon. the EARL OF FIFE, *relative to his Plantations**.

SIR,

Plants above 100 acres a year.

I request you will lay this letter before the Society for the Encouragement of Arts, &c. as I feel it my duty to convey any information to them, respecting my plantations, from the grateful sense of the honour they have done me. I have continued every year, since I last wrote to the Society, to plant above one hundred acres: my plantations now, in the counties of Banff, Aberdeen, and Murray, amount to about thirteen thousand acres. I have always recommended to planters to be very sparing in pruning trees.

Trees should be very sparingly pruned.

Oaks.

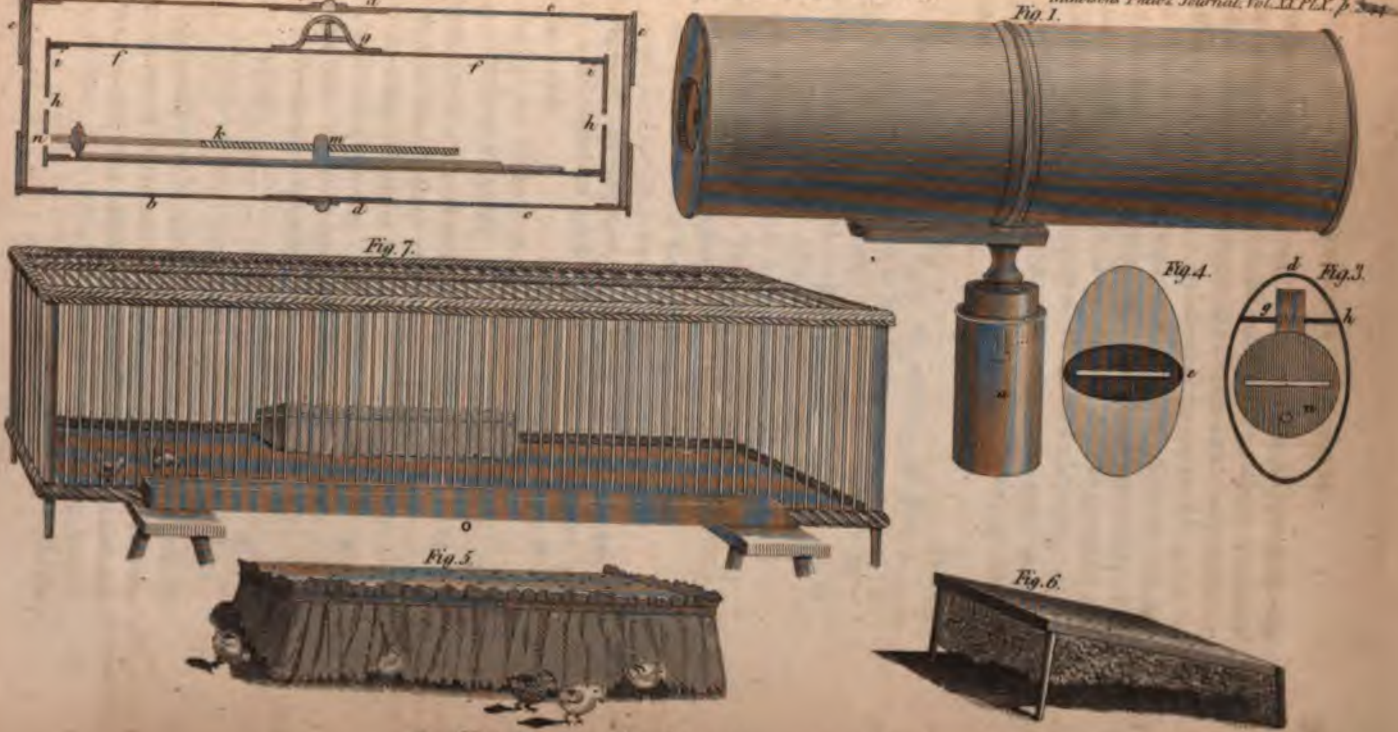
I have the pleasure to observe, that on the highest grounds in Duff-House Park, even where exposed to the sea, by cutting down firs and other trees, where they interfere with each other, the oaks and other close-grained timber trees rise vigorous and healthy, and will be very valuable, the oaks in particular. The silver fir and larch also grow to a great size. I was under the necessity of cutting down two silver firs and larches, where they prevented the growth of other trees; I directed them to be sawed up—The boards of the larch have been made into tables, and are

Larch wood handsome.

* Trans. of the Society of Arts for 1807, p. 1.

very

Mr. Richard Drew's Balance Level.



Mrs. D'Oyley's Method of rearing Poultry.

very handsome. Those of the silver fir have been used as flooring to two rooms in Delgany Castle, where the fir had decayed, and are remarkably white and finely polished. The trees in question were about forty years old. Silver fir planks.

There was a very high wind the 25th of December last, which blew down a great many trees upon my estate. Particularly a silver fir in the woods on the low grounds near Duff-House, which appeared to be well sheltered. It was planted by me in the year 1756, and had a most venerable appearance. The dimensions were as follow, as attested to me, viz. Dimensions of a silver fir at 50 years.

	Ft.	In.
Length of the trunk from the surface of the ground, until divided in five limbs - -	7	0
Girth at surface of ground - - - -	9	7
Girth immediately below where the limbs set off	8	6
The five limbs are all of the same height, except No. 1, which divides into two branches before it reaches the top. These are only a few inches shorter than the others, which are 42 feet 6 inches from where they leave the trunk, the length of which is 7 feet, therefore, when added together, the height of the tree, is - -	49	6
No. 1. Measure of girth where it sets off from the trunk - - - - - -	5	3
And at the distance of 8 feet divides itself into two large branches.		
No. 2. Girth where it sets off from the trunk	4	0
And at the distance of 23 feet 4 inches from starting, measures 2 feet.		
No. 3. Girth at starting - - - -	3	10
This, and the two other branches, No. 4 and 5, gradually decrease towards the top.		
No. 4. Girth at starting from trunk - -	3	7
No. 5. Girth at ditto ditto - - - -	3	3

The tree alluded to has a great deal of wood in it, which I have ordered to be manufactured for different purposes. There are pineaster larger, but their wood I conceive not to be so fine. The other trees are thriving and well-fenced. Pineaster.

Little

Little trouble is occasioned by keeping the fences in repair. I do not recommend the planting of acorns, but rather procure them from nurseries, at two or three years old.

Seedling larch. I think seedling larch thrives best when planted in moors, and this also thins the seed beds, from which so many may be taken and transplanted into nurseries, and planted out the second year after.

Scotch firs. Hard woods. I raise very few Scotch firs, as I buy them from nurserymen, at ten-pence per 1200. I continue to have nurseries of all the different hard woods, near my plantations, and which I find answer better than what are purchased from nurserymen. In general they are planted too near each other in their nurseries, and not being removed in time, the roots are seldom so good, which I think I have stated in my former letters.

I am, Sir,

Your most obedient humble servant,

FIFE.

IX.

Remarks on the Advantages derived from Plantations of Ash Trees, by David Day, *Esq, of West-hill, near Rochester**.

Former account of planting ash trees. IN the first volume of the Society's Transactions for 1783, page 109, will be found a detailed account of the experiments which Mr. Day had made to the years 1779 and 1780 in planting Ash trees; the present account points out their subsequent management.

Mr. Day has deposited with the Society a minute account of the expenses to which the following statements refer, and which may be inspected at the Society's house.

* Abridged from Trans. of Society of Arts for 1807, p. 4. The silver medal of the Society was voted to Mr. Day.

SIR,

SIR,

IF you think the following information, relative to my plantations of ash trees, likely to be of advantage to the public, I wish to lay it before the Society of Arts, &c. The Rewards I received from the Society have stimulated me to exertions in this line, and I have been very successful. I have declined all business but that of raising ash trees for my own amusement, and for improvement of the landed interest; and I flatter myself, that I know it as well as any man in the kingdom. I am so certain of the success attending ash plantations, that I am willing, on landed security being given me, to advance any sum as far as thirty thousand pounds, on having the execution of such improvements under my own inspection, either jointly, or on the owner's account. I have travelled over a considerable part of England, and was sorry to see such a waste tract of land as Bagshot Heath, when I know it might be improved by cultivation or planting, as has really been done at Farnham, and many other commons in the kingdom. Where there are, at present, wild or uncultivated woods, I would recommend to grub up the old wood, and either put the land in tillage, or plant it properly with fresh wood, which would produce four times as much both in timber and underwood.

Stimulated to exertion by the rewards from the society.

Bagshot heath might be improved.

Wild or uncultivated woods.

I have made from the underwood of some of my plantations 94*l.* per acre, at only ten years growth, and I am now falling some plantations, of which the underwood alone will produce me 150*l.* per acre, exclusive of the expense of falling. It cannot be expected that noblemen or gentlemen, brought up in expectation of possessing large estates, can have a knowledge of improvements like the executive man, and they are deterred from them by the impositions they meet with in attempting their execution. Few servants will exert themselves properly in improvements without having an interest in it themselves: But my plan is no speculation; I know from long experience it will yield an ample profit to the persons who engage in it with attention. When the plantations are once put in order, they require but little to be done afterwards, and, therefore,

Profit of plantations of ash.

Valuable portions for daughters.

therefore, are good estates for parents to give their femal children, as the wood will always find its value from th buyers, when ready for falling, without any trouble or ex pense to the owner.

I am, Sir,

Your obedient servant,

DAVID DAY

Statements and profits.

Mr. Day next proceeds to a detailed account of the ex penses and receipts on various plantations, of the profit from which the following is a summary.

acres	years				
2 acres in	30 years produced a clear profit of	135	1	
-	- - - - - - -	254	1	
1½ -	20 - - - - -	121	16	
7 -	10 - - - - - -	88	14	
6½ -	22 - - - - -	108	1	1
¼ -	24 - - - - - -	27	11	
5 -	23 - - - - -	501	2	
¼ -	24 - - - - - -	19	14	
2 -	10 - - - - -	23	19	
2 -	25 - - - - - -	59	18	1
¼ -	28 - - - - -	73	8	
11 -	19 - - - - - -	56	8	
6 -	23 - - - - -	56	6	

These 6 acres were planted with ash and chesnut; an the 11 acres mentioned in the preceding line were in t hands of a farmer as a tenant, whose cattle, being pe mitted to graze among the plants, did them much damage

Method of raising ash trees.

The following is Mr. Day's method of raising ash tree given in his own words.

Methods of raising Ash Trees.

Choice of seed.

I carefully procure, from good straight well-proportion ash trees, the ash keys, as they are commonly called, pods containing the seed, betwixt Christmas and the midd of February. Having, as soon as the ash keys are collecte prepared a hole in the ground, about three or four fe deep, I lay a bed of sand, a few inches deep, at the bo tom of the hole; upon that I place a layer of ash key abo

Keeping it.

about two inches thick; these I cover with sand about the same thickness, to preserve the keys from heating, and then proceed with alternate layers of the keys and sand till the hole is full. They are suffered to remain in this state till the beginning of the month of March of the following year, when they should be taken out for sowing. The keys will be found in a swelled state, ready for vegetation. The land being properly prepared, drills should be made in it as for sowing pease, and the keys laid regularly therein, and covered up with earth.

Sowing.

In about six weeks the young plants will appear above ground, and should be kept perfectly clear from weeds by hoeing.

Hoeing.

In the month of March of the next year they should be planted out in rows, a foot wide, and the plants placed three or four inches asunder in the row. In this state they are to remain for two or three years, when they will be in a proper condition for planting out into the land, where they are to remain.

Planting ou

For planting out where they are to remain, the land being previously well ploughed the preceding autumn, and a good loamy soil, not too wet or stiff, the ground is to be opened by the plough into drills about two feet apart, and the plants placed in each other drill or row, so that the rows of ash are four feet apart, and the plants in the drill two feet asunder. The drills should be 10 or 12 inches deep. A man, who sets the plants, places each upright in the drill, draws the earth to it with his foot, and treads it well in. Where a plant with a larger root than common is found, the man with a small hoe or pricker makes a hole within the drill, a little deeper than usual, to hold the plant, but this is not often necessary.

Final planti

A statute acre will contain 5400 ash plants, and one man can plant 1000 or 1200 plants in a day.

The intermediate row between the plants may be either set with beans or potatoes, or may be left open advantageously to serve as a drain to keep the young plants dry.

In the second year the plants should be stubbed, or cut close to the ground with a bill; the produce serves for bavins or fire wood, and pays the expense of rent and cutting

Stubbing th plants.

Crops.

ting. From the stubs thus left in the ground the regular crops of ash are produced, and are fit for falling every ten years.

The ash plants are usually fallen betwixt Christmas and March, and the wood sorted into poles of three denominations; viz. best, second, and third hop-poles; beside stakes, edders, and bavins. The bavins will amply pay the expense of falling. The best hop-poles are worth, at present, forty shillings per hundred, the second quality twenty-five shillings, and the third ten shillings. Stakes and edders are about two shillings and sixpence the hundred.

Uses of the wood.

Other uses.

When the plants remain uncut for twelve or fourteen years, the ash plants are fit for other purposes, such as wood proper for wheelrights, and broad hoops for coopers, beside hop-poles, &c. as before-mentioned.

Nurseries.

When the plants have been two years in the nursery beds, and ready for planting out, they are worth from six shillings to ten shillings the hundred according to their quality.

The ash plants I raised from 1763 to 1778, were	442484
I raised and sold from 1778 to 1807 -	156320
I have now ready for sale - - -	126096
Total of plants I have raised	724900

Cheapness of planting.

I will engage, provided the land is prepared by my directions, to plant ash for one third less money than by any other mode of cultivation yet known; and for all plants that die in such a case, provided they are in new plantations, I will give plants gratis to replace them the succeeding year.

X. *Chemical*

X.

Chemical Examination of a Sparry Iron Ore, sent to Mr. GUYTON *by* BERGMAN. *By Mr.* COLLET-DESCOTILS *.

THE analyses of iron spar, that have been lately published, having exhibited results considerably different from those obtained by Bergman, it was to be wished, that some of the species on which that celebrated chemist had operated might be subjected to a fresh examination. In fact this was the only method by which it could be known, whether these differences arose from the composition of the ores themselves, or from mistakes in the analysis. Mr. Guyton, who had received from the Swedish chemist a small specimen of the very ore, that had been the principal subject of his examination, having the goodness to break off some pieces from it, and entrust them to me to analyse, I have executed the task with all the attention I could possibly pay to it. The small quantity of iron spar I had at my disposal, it being only 388 cent. [60 grs.], and the method I employed, not allowing me to ascertain the proportion of the volatile principles, I confined myself to the investigation of the nature and quantity of the fixed; and I conceive it necessary, to relate at large the means I employed, that the chemical reader may be enabled to judge of the degree of confidence to be placed on my results; previously giving a brief description of the specimen on which I operated.

Recent analyses of iron spar contradictory to Bergman's.

A piece of the same ore examined.

Its specific gravity, taken by Mr. Guyton, was 3·693.

Its physical characters.

Its colour was brownish yellow.

It was scarcely translucid.

Its crystallization was a little confused: its laminæ very small, and a little twisted.

This ore, reduced to powder, was dissolved with effervescence in sulphuric acid diluted with water; and I took care to employ no more than was necessary, so that the liquor was without excess of acid. Some insoluble matter remained, weighing 1 decig. [1·544 grs.], which was found to be silex.

Dissolved in sulphuric acid.

* Annales de Chemie, Vol. LVIII, p. 149.

The

Evaporated and redissloved.

The solution, on several successive evaporations, t ed crystals of green sulphate. Only a few small s sulphate of lime were formed. The last portions of affording no more crystals, were added to the crystal had been separated, and the whole diluted with a q of water more than sufficient for their complete solut

Sulphuretted hidrogen added.

Water of sulphuretted hidrogen occasioned no precip in this solution: it merely destroyed its transparency, the case in solutions of iron that contain but a small quan of red oxide.

Precipitated by hidrosulphuret of ammonia.

The hidrosulphuret of ammonia, afterward added, casioned a very copious black precipitate, which was se rated by the filter, and washed in cold water.

Precipitate dissolved in aqua regia.

The precipitate detached from the filter was treated w aqua regia. I neglected to burn the filter, and treat th ashes in the same way, which may have occasioned a litt loss of the metallic principles; for this very fine precipita easily insinuates itself into the paper.

Solution decomposed by carbonate of potash.

The nitromuriatic solution diluted in water and filtered was decomposed by saturated carbonate of potash. The ferruginous sediment was redissolved, while still wet, by weak acetic acid, and by successive evaporations the acetate of iron was entirely decomposed. The sediment, collected on a filter, was dried with a red heat, and weighed 188 cent. [29 grs.]. The colourless liquor, separated by the filter, was decomposed by saturated carbonate of potash: it did not become turbid, and it was added to that, which arose from the decomposition of the nitromuriatic solution. From this carbonate of manganese was soon thrown down by boiling, which, when washed, and dried at a red heat, was converted into brown oxide, and weighed 7 cent. [1·08 gr.].

Oxalate of ammonia added.

The liquor separated from the metallic precipitate obtained by the hidrosulphuret of ammonia was mixed with a small quantity of oxalate of ammonia, which did *not* occasion in it any sensible precipitation. On evaporating afterward, a white sediment formed, which was separated. This sediment, heated in a small porcelain capsule, burned with a blue flame, and left a residuum, which, after being heated red hot, was found to weigh 2 cent. [0·308 gr.]. It had all the characters of lime.

The

The clear liquor was then evaporated to dryness in a platina crucible, and the residuum heated red hot. The ammoniacal salts being expelled, there remained a salt of the weight of 21 cent. [3·243 grs.], which was sulphate of magnesia. This quantity of 21 cent. gives at least 77 milli. [1·189 gr.] of earth, supposing the crystallized sulphate to contain 19 per cent of base; for that which has been heated red hot must have lost some of its acid, and it is necessary to add a little to the solution, to make it crystallize. Solution evaporated and heated red hot.

On reducing the products above-mentioned to hundredth parts, we shall have

Fragments of quartz - - -	2·58
Red oxide of iron - -	48·45
Brown oxide of manganese - -	1·80
Lime - - - - -	0·52
Magnesia - - - - -	1·98
	55·33

Component parts.

The remainder is carbonic acid, water, and loss.

From this result it appears, that Bergman did not examine with sufficient care the nature of the earthy principles contained in the iron spars he analysed; and it is very probable, that he examined other ores, in which magnesia was contained in still larger proportion, and mistaken by him for lime *. Magnesia mistaken for lime.

XI.

Chemical Examination of the Alum Ore of Tolfa, and the Earthy Aluminous Schist of Freyenwalde. By Mr. KLAPROTH †.

ALUM, a substance so indispensable in dyeing and several other arts, is a triple salt, composed of sulphuric acid, alumine, and potash, with an excess of acid. It is Alum an artificial production from ores.

* In the last number of our Journal, p. 314, an analysis of two varieties of iron spar was given, which corroborates the fact of magnesia having been mistaken for lime.

† Journal des Mines, No. 117, p. 179. First published in the Berlin Chemical Journal, vol. VI.

obtained

obtained from various earths and stones, which contain the elements necessary to its formation in a more or less perfect state, and are included under the name of alum ores. Thus the alum of the shops is an artificial production.

Native.

Nature it is true presents us with alum completely formed in some volcanic countries, but it is in so small a quantity, as to be altogether insignificant compared with the great demand for it. Among the native alums of volcanic countries that of the alum grotto at cape Miseno, near Naples, is particularly to be distinguished. This is continually efflorescing on the inside of the cavern in small tufts, composed of little, short filaments of a silky lustre, sometimes intermixed with granular crystals. From the results of my examination, which have been published some years, it is well known, that the greater part of this native salt is a perfect alum, that is to say, it has from nature not only the sulphuric acid and earthy base, but likewise the third essential constituent principle, potash.

At Cape Miseno.

Alum of the ancients.

It appears, that the alum we now use was not known to the ancients; and that the *alumen* of the Romans, as well as the στυπτηρία of the Greeks, was a native sulphate, arising from the decomposition of pyrites, and consequently not differing from their *misy* and *sory*.

First made in the Levant.

The art of extracting and preparing alum came to us from the Levant. The most ancient of the alum works known to us is that of Rocca in Syria, now called Edessa; whence the term *alumen Roccæ*, vulgarly rock alum. All the alum used in Europe in the middle ages was brought from the Levant.

Roch alum.

Introduced into Italy.

In the fifteenth century some Genoese, who had learned in the Levant the mode of fabricating it, were fortunate enough to discover ores of it in Italy, and to extract it from them. John of Castro is recorded in history as the first, who discovered the ore of Tolfa. To this discovery he was led by the large quantity of holly growing there; as he had observed in the Levant, that the mountains from which alum was taken there were covered with this shrub.

Holly abundant on aluminous soils.

The manufactures of this salt succeeded so well and so speedily in Italy, that pope Julius II prohibited its importation

tation from the Levant, because it annually drew large sums of money to Turkey. This prohibition increased the prosperity of the Roman alum works.

The following is a brief account of the method employed at Tolfa, near Civita Vecchia. The ore is blown up with gunpowder: it is separated from the pieces of the rock, that adhere to it: it is calcined in furnaces, nearly in the same manner as lime is burned: in six or seven hours, being sufficiently calcined and friable, it is taken out, and laid on pavements of a long shape, surrounded with walled trenches: on these it is laid in heaps of a moderate height, which are watered for forty days with water from the trenches. The ore being thus decomposed, it is boiled in large caldrons; and when the water is saturated to a certain point, it is poured into the crystallizing pans; where, after it is cold, it deposits the alum in large crystalline masses. Method of making it at Tolfa;

Alum is obtained in a very different manner at Solfaterra, near Puzzuola. Here nature acts synthetically. Fumes pregnant with sulphurous and sulphuric acid are continually issuing from little crevices in the volcanic soil of this place, the former of which deposit a concrete sulphur; the second gradually penetrate the ancient lavas, which are of an argillaceous nature, combine with their alumine, and thus form an alum ore, which afterward affords by lixiviation and crystallization a very pure alum. and at Solfaterra.

In the sixteenth century the art of fabricating alum spread into several parts of Europe; after it had been discovered, probably by accident, that various sorts of argillaceous schists, impregnated with carbon or bitumen, and subsequently termed aluminous schists, would furnish alum when properly treated; and the alkali, which did not naturally exist in them, was added during the process. The first works of this kind established in Germany appear to have been those of Commotau in Bohemia, and Schwensal in Saxony. Introduced into other parts of Europe. Germany.

Subsequently, that is in the beginning of the last century, an alum manufactory was commenced at Freienwald, in Brandenburg. At present it belongs to the grand Orphan School at Potsdam, and furnishes annually four hundred tons of alum. Manufactory at Freienwald.

Preparation of the ore.

The aluminous schists, from which alum may be obtained, must undergo a process preparatory to their lixiviation. In the aluminous schists properly so called, which are hard, of a stony texture, and contain a great deal of pyrites, the preparatory process consists in roasting. But for the softer alum ores, such as that of Freienwald, exposure to the air is sufficient. When the ore is extracted from the mine, it is placed in large heaps, sloping to a ridge like the roof of a house, and left exposed to the open air for a year or more. When its decomposition, which is particularly promoted by damp air, is sufficiently advanced, it is distributed into long flat troughs, and lixiviated. When the water is sufficiently saturated with the salts, which are sulphate of alumine and sulphate of iron, it is carried to the manufactory, and boiled in leaden caldrons, till the proof liquor taken out becomes on cooling a crystalline mass of the consistence of honey. During the long boiling of the lixivium, the greater part of the sulphate of iron is decomposed, the iron passes to a higher degree of oxidation, in which state so much of it cannot be dissolved in sulphuric acid, and it is deposited in the form of brown oxide. When the lixivium is sufficiently boiled down, it is carried to the settling troughs, and as soon as it has grown clear by standing a little, it is drawn off into other troughs, where it is mixed with the quantity of potash necessary for making it into alum.

Lixiviation.

Oxidation of the iron.

Potash added.

This obtained from soap-boilers refuse.

At Freienwald, as at most alum works, they use for supplying the alum with this potash the saline mass obtained from soap manufactories, where soap is made with an alkaline lie and muriate of soda, by boiling the spent lie to dryness. The muriate of potash contained in this saline mass is decomposed the instant it is mixed with the aluminous lixivium: the potash unites with the sulphate of alumine, and forms alum, which can no longer continue in solution in the concentrated lixivium, and is precipitated in the form of small crystalline grains, known by the name of alum meal. The muriatic acid, thus set free, lays hold of the oxide of iron, and prevents its falling down with the alum. Instead of the saline mass from the soap-makers, matters containing sulphate of potash might be employed, as the residuum

Other matters might be used.

residuum left after the distillation of nitric acid, glass gall, &c. The alum meal is washed with cold water, redissolved afterward in a small quantity of boiling water, and lastly drawn off into large wooden vessels, where it is left to crystallize slowly. Crystallization.

I shall now proceed to the proper object of this essay, namely, the chemical analysis of the alum stone of Tolfa, and of the earthy aluminous schist of Freienwald. Analysis of the alum ore of Tolfa.

I. *The Alum Ore of Tolfa.*

The alum stone of Tolfa in its natural state contains the three essential constituent principles of alum, considered as a triple salt; sulphuric acid, alumine, and potash. The earth in which it is found is probably of volcanic origin, and has been altered and whitened by the vapour of sulphuric acid. In this it exists in irregular veins, and in nodules. The harder and heavier it is, the richer in alum it is presumed to be. Some naturalists, as Monnet and Bergman, have supposed it contained sulphur, which was afterward converted into sulphuric acid by the process of roasting. But Dolomieu and Vauquelin have shown, that this acid is ready formed in the ore, which will be farther confirmed by what I shall say. Contains all the essential parts of alum. Acid ready formed in the ore.

The alum stone employed in my analyses was of a pearl gray, that is, gray with a violet tinge; in amorphous masses; dull, with a few shining points, or having very little lustre; of an unequal fracture approaching to shelly; a little translucid on the edges; hard, not adhering to the tongue, and heavy. Physical characters of this ore.

A. Two hundred grains were strongly roasted in a small retort with its proper apparatus. An aqueous liquor passed over, highly loaded with sulphuric acid, and accompanied with a smell of sulphurous acid, but without a particle of sulphur. The loss of weight was twenty nine grains. Roasted.

B. Two hundred grains were gently heated, so that the loss of weight could proceed only from water expelled. This loss was six grains. Water expelled.

C. *a.* Two hundred grains were reduced to fine powder, mixed with twice as much carbonate of soda, and the whole subjected to the action of a fire, at first moderate, but afterward Fused with soda,

terward increased so as to fuse it. The mass when cold had the appearance of a white enamel. It was well powdered, muriatic acid poured on it to supersaturation, and evaporated to dryness. The residuum, mixed with water and diluted, left behind silex, which after being heated red hot weighed 113 grains.

muriatic acid added,

and diluted with water.

Part of the solution precipitated by muriate of barytes;

b. The muriatic solution was divided into two parts. Into one of these was poured a solution of muriate of barytes, and sulphate of barytes was precipitated, which after being heated red hot weighed 50 grains; indicating 16·5 grs. of concrete sulphuric acid.

the other by ammonia.

c. The other half was precipitated by ammonia, which threw down the alumine. This when purified, washed, and roasted, weighed 19 grains.

The ore heated with nitrate of barytes, dilute sulphuric acid added, and precipitated by ammonia.

D. A hundred grains of the ore were mixed with 200 grs. of crystallized nitrate of barytes, and heated red hot. The mass was pounded, mixed with water, and supersaturated with sulphuric acid. After evaporating till the saline mass was moderately dry, it was diluted in water, boiled, neutralized with ammonia, and filtered. The liquor being evaporated, and the residuum heated red hot in a platina crucible, left seven grains of sulphate of potash, which included four grains of pure potash.

Its component parts.

According to this 100 parts contain

Silex - - - -	56·5
Alumine - - -	19
Sulphuric acid - - -	16·5
Potash - - -	4
Water - - - -	3
	99·

These as given by Vauquelin,

These component parts are the same in kind as those found by Mr. Vauquelin, who gives them as follows:

Silex - - - -	24
Alumine - - -	43·92
Sulphuric acid - - -	25
Potash - - -	3·08
Water - - - -	4
	100·

The

The difference between these analyses in regard to the respective quantities of the several component parts must have arisen, no doubt, from a difference in the composition of the specimens.

differ in their quantities.

II. *Earthy Aluminous Schist of Freienwald.*

The mineral that furnishes the alum of Freienwald owes its origin unquestionably to the vegetable kingdom, and appears to be produced by an alteration of brown coal. It forms a considerable stratum amid the alluvial formation at Freienwald, which is traversed by galleries for its extraction. At coming out of the mine it is of a brownish black, tender or friable, and very slightly shining. Its fracture in the great is imperfectly slaty; in the small, earthy. When rubbed it takes a lustre inclining to that of wax. It belongs to that species of the argillaceous genus, that is designated in the systems of mineralogy by the term aluminous earth *(alaunerde)*. This mineralogical term must not occasion it to be confounded with the simple substance known by chemists under the name of earth of alum *(alaunerde)*, and it is to prevent this mistake I here employ the denomination of earthy aluminous schist.

Alum ore of Freienwald of vegetable origin.

Hitherto this mineral, as well as the true aluminous schist, has been considered as a clay impregnated with bitumen and pyrites. It is indeed true, that the earthy schists, and still more those that have the consistence of stone, very frequently contain pyrites: but such ores afford only a very ferruginous alum, and are consequently less fit for the fabrication of this substance, than for that of vitriol.

Mistakenly supposed to contain bitumen and pyrites.

The following experiments, made on alum ores of the first quality, will show, that the sulphur they contain is not combined with the iron in the state of pyrites; but that it appears to form a peculiar combination with carbon.

The sulphur combined in a peculiar way with carbon.

A. *a.* A thousand grains of the ore, in the state in which it was extracted, were put into a phial with twenty ounces of distilled water, and boiled for an hour; when the liquor was filtered off, and the residuum lixiviated. What passed through the filter was colourless, did not perceptibly change blue vegetable tinctures, and had a vitriolic taste.

The ore boiled in water.

b. Half

Half the solution decomposed by muriate of barytes,

b. Half of this was decomposed by a solution of muriate of barytes, and sulphate of barytes was formed, which, after being heated red hot, weighed 23 grains. This precipitate being separated from the liquor, prussiate of ammonia threw down another of prussiate of iron, weighing 40 grains.

the other half by oxalate of ammonia.

c. To the other half oxalate of ammonia was added. It became a little turbid, and assumed a pale yellow colour, which probably arose from a small quantity of oxalate of iron. It then gradually grew clear again, a precipitate falling down, which, after having been heated red hot, weighed 2·5 grains, and was found to be lime contaminated with iron.

Proportions of the sulphates of lime and iron determined.

Thus what the ore had yielded to the water, in which it had been boiled, consisted of sulphate of lime and sulphate of iron, the proportions of which may be determined as follows. A thousand parts of ore produced 46 parts of sulphate of barytes, which contain 15·18 parts of concrete sulphuric acid. Of these 7 parts are required to neutralize the 5 parts of lime; and thus, including the water of crystallization, we may admit 15 parts of gypsum, or sulphate of lime, in the ore. The 8·18 remaining parts of sulphuric acid with 8·5 parts of iron will give about 18 parts of vitriol of iron at the state of decomposition.

The ore boiled with carbonate of soda and precipitated by muriatic acid.

B. Two hundred grains of ore, and 400 grains of dry carbonate of soda, were put into water, and boiled. The liquor when filtered was of a very deep blackish brown colour. Muriatic acid was gradually poured in, which afforded no indication of sulphuretted hidrogen gas; but a muddy sediment was formed, of a blackish brown colour, and occupying considerable space, which, when collected on a filter and dried, weighed twelve grains. Heated in a platina crucible, it burned, without emitting any sensible smell of sulphur, and left behind one grain of white alumine.

Ore digested in muriatic acid.

C. Two hundred grains were digested in muriatic acid. The slightest indication of sulphuretted hidrogen gas was not observable, either by the smell, or by holding against the mouth of the vessel paper, on which I had written with solution of acetate of lead. The acid appeared to display

but

but little action on the ore. On pouring nitric acid on it drop by drop, nitrous gas was evolved, and the black colour of the ore changed to brown. The filtered solution was of a golden yellow; and muriate of barytes threw down from it a copious precipitate. This, which was sulphate of barytes, being collected and heated red hot, weighed 54 grains.

Nitric acid added, and muriate of barytes.

D. *a.* A thousand and two grains of the ore, not yet freed from the humidity it contains in the mine, were put into a glass retort furnished with a pneumato-chemical apparatus. Two hundred and twenty cubic inches of gas were evolved, which was a mixture of sulphuretted hidrogen gas and carburetted hidrogen gas. If a candle were brought into contact with it, it took fire, and burned with a blue flame. When shaken in a vessel containing water, half of it was absorbed. A solution of lead, poured into the water impregnated with it, afforded a precipitate of a deep brown, which was sulphuret of lead.

Ore distilled, gave out sulphuretted and carburetted hidrogen.

b. The fluid that passed over weighed 133 grains. It was aqueous, yellowish, and turbid with slight flocks of sulphuretted carbon. Its smell was that of sulphuretted ammonia, diluted with a great deal of water. Litmus paper, that had been reddened by an acid, it turned blue, and it emitted a white vapour, on bringing near it a glass rod wetted with fuming muriatic acid. A drop was let fall into a solution of lead, and the metal was precipitated brown. It was neutralized by a few drops of muriatic acid, and became slightly milky. On being filtered and evaporated two grains of sal ammoniac were obtained.

The fluid contained sulphuretted ammonia.

c. The residuum left in the retort weighed 750 grains. It had the appearance of a black coally powder. Being burned on a test it left 90 grains, which were the carbon consumed.

Residuum contained carbon.

d. The fifth part of the remaining 660 grains, or 132 grains, was roasted with twice its weight of caustic soda. The mass when cold was of a greenish brown, and gave a light green tinge to the water with which I mixed it. I supersaturated it with muriatic acid, evaporated, diluted it again with water, and filtered. The silex was left behind. This, after being heated red hot, weighed 80 grains.

Silex precipitated from a portion of it.

e. The

Alumine.

e. The solution that had passed through the filter I precipitated by carbonate of potash, washed the precipitate, and boiled it in a lixivium of potash, which became loaded with alumine. This earth being precipitated by muriate of ammonia, washed, and heated red hot, weighed 32 grains.

Sulphate of lime.

f. The brown residuum, that remained in the alkaline lixivium, was dissolved in sulphuric acid, and evaporated to dryness. During the evaporation sulphate of lime was deposited, which, carefully collected, weighed two grains. The dry mass was strongly roasted, and then lixiviated.

Oxide of iron.

The oxide of iron, collected on the filter, was dried, moistened with a little oil, and heated red hot in a close vessel, when it yielded 14·5 grains of oxide of iron attractable by

Magnesia.

the magnet. The remaining liquor, decomposed during ebullition by carbonate of potash, gave some slight indications of carbonate of magnesia.

Water expelled by heat.

E. *a.* One hundred grains * were put into a small glass retort, which was placed on a sand heat, and the fire cautiously increased, lest any gas should be evolved, or any perceptible decomposition occasioned, and that nothing but water might be raised from it. The quantity expelled was 21·5 grains. It had a very slight opal tinge, and a very faint smell of sulphuretted hidrogen. A very slight coating of sulphur too was deposited in the neck of the retort.

The ore burned without flame or smoke.

b. The ore being dried was burned on a test; when the combustion proceeded without flame or smoke, and emitted but a slight sulphurous smell. The loss in weight, which was 45 grains, represents the quantity of sulphur and charcoal burned, and perhaps too a small portion of water, that was left in the ore.

Magnesia precipitated.

c. The residuum was dissolved in a mixture of 200 grains of sulphuric acid, and 400 of water, evaporated to dryness, and kept at a strong red heat for half an hour. The residuum was lixiviated, filtered, and precipitated with ammonia, when 0·5 of a grain of magnesia were obtained.

* This is apparently an errour of the press. According to the proportions of the constituent principles given at the end, it must have been two hundred grains. *F. Ed.*

d. The

Sulphate and muriate of potash.

d. The liquor was evaporated to dryness, and the residuum heated till no more white fumes were expelled. What remained weighed 4·5 grains. It was a neutral salt, formed of a mixture of sulphate and muriate of potash. As this last salt must necessarily have been completely formed in the ore, we may admit too, that the potash of the former was not free in it, but formed a real component part of it in the neutral state. Till experiments on a larger scale shall have enabled me to determine more accurately the proportions of these two salts, I shall reckon that of sulphate to that of muriate as three to one.

Corrections of received notions.

F. The results of the experiments above given will serve to rectify some of our chemical ideas respecting the earthy aluminous schist of Freienwald, and those of a similar nature.

The ore contains carbon, but no bitumen.

1. In their composition there is carbon only, but not bitumen; for they afford no bituminous oil by distillation, and when roasted in open vessels they burn like charcoal without flame or smoke.

The sulphur united with carbon in a peculiar way: and not forming pyrites.

2. The sulphur of the ore, which becomes oxigenized during its exposure to the air, and thus forms the sulphuric acid necessary for the production of alum, is not combined in it in the state of pyrites, exclusively of any pyrites mixed with the ore accidentally, but is intimately united with the carbon, and this in a manner with which we are not yet well acquainted. With the best lenses we cannot discover the smallest atom of pyrites in the ore, either in its natural state, or after it has been triturated and washed through the sieve with care*. In this state of combination with carbon the sulphur is protected against the solvent power of alkalis, and gives no sulphuretted hidrogen gas with muriatic acid.

Difficult to determine the proportions of

G. As to the determination of the respective proportions of the constituent principles mentioned, there is some diffi-

Fire damp not from pyrites.

* I have observed in several coal-mines, particularly those of Anzin, a fact, that has probably some connexion with this mentioned by Mr. Klaproth. The coal that produces fire damps does not contain any pyrites, at least perceptible to the eye; and in the same places the coal that contains a great deal of pyrites is wrought without the least danger.

 culty

Sulphur and carbon.

culty in it, arising chiefly from the intimate union between the carbon and the sulphur; as these two substances cannot be separated in the dry way, without new gaseous compounds being formed.

Alumine.

The essential parts of the mineral, as an alum ore, are alumine and sulphur. The ordinary processes of analysis give us directly 160 parts as the quantity of alumine in 1000 of the ore.

Sulphur.

The sulphur not being obtainable in a separate state, we must deduce its quantity from that of sulphate of barytes obtained in treating the ore by nitric acid. According to what has been said (in C), 1000 parts of the ore produced 270 of this sulphate. From this quantity 46 parts are to be subtracted, which were furnished by the vitriol and gypsum, and 20 by the sulphate of potash, admitting 15 of this sulphate in 1000 of the ore. Thus there remain but 204 parts of sulphate of barytes produced by this sulphur: but 204 parts of this salt contain 90·75 of sulphuric acid of the specific gravity of 1·85, or 67·5 of concrete acid, which are produced by the oxigenation of 28·5 of sulphur.

Carbon.

And if (according to E *b*) the sum of the sulphur and carbon may be taken at 225, on deducting 28·5 for the sulphur we shall have 196·5 for the quantity of carbon.

1000 p. ore might produce 216 alum.

H. Admitting that 1000 parts of crystallized alum, decomposed by muriate of barytes, produce at a mean 945 of sulphate of barytes, we shall find, that the 28·5 of sulphur contained in 1000 of ore may afford 216 parts of alum, provided the proper quantity of potash be added. The component parts of the ore, that produce them, are not a fifth part of the mass.

Less obtained from defects in the process.

If the quantity of alum obtained, or even that might be obtained in the manufactories, be much less than I have mentioned, this arises from the imperfection of the process employed to produce the efflorescence of the ore during its exposure to the air. The oxigenation of the sulphur, and consequent formation of sulphate of alumine, takes place only on the surface of the lumps, and of course the greater part of the ore remains undecomposed.

Component parts of the aluminous schist.

I. From the preceding experiments we may infer, that 1000 parts of the earthy aluminous schist of Freienwald contain

Sulphur

Sulphur	G.		28·5
Carbon	G.		196·5
Alumine	D. *e*.		160
Silex	D. *d*.		400
Black oxide of iron, with a slight trace of manganese	D. *f*.	72·5	
From which subtract for the vitriol		8·5	
		64	64
Vitriol of iron	A. *c*.		18
Sulphate of lime	A. *c*.		15
Magnesia	E. *c*.		2·5
Sulphate of potash	E. *d*.		15
Muriate of potash	E. *d*.		5
Water	E. *a*.		107·5
			1012·

It is very possible however, that the quantity of some of these component parts may be capable of being determined with more accuracy. As to the excess of about one per cent, which the sum total shows, this may be considered as of little importance in an analysis like the present.

XII.

On the Effects of Galvanism on Animals. In a Letter from Mr. John Tatum.

Dear Sir,

My two papers on galvanism having met with an insertion in your Journal, induces me to send a third, containing galvanic experiments, some of which I presume will be new to most of your readers, as I believe no one has performed them but myself. Galvanic experiments.

After having killed two frogs, one by electricity, and the other by immersion in carbonic acid gas, and dissected them in the usual manner, I endeavoured to excite them by a galvanic trough of 50 plates, containing 350 inches surface, but no muscular contractions ensued. I did not (as Two frogs killed, one by electricity, the other by fixed air, could not be excited.

is generally the case) confine my experiments to the infer
parts only of the frog, but made them on the superior al

The first moistened with oximuriatic acid, without effect.

I then moistened both upper and lower extremitie the frog killed by electricity with oximuriatic acid, and i mediately applied the positive and negative wires of above trough, but with as little success.

But six hours after it was convulsed by a single pair of plates.

Having left them on the table, to attend to some ot experiment in another room, I did not return to rem them till about six hours after the experiment, when I w much surprised to observe the head of the latter frog app more healthy than when I left it. It being late in the ev ing, I began to lament, that I had emptied and cleaned trough, as I wished to try its effects a third time; but fr the appearance I was tempted to try the effect of a pair zinc and silver plates of $1\frac{1}{2}$ in. diameter, and the conv sive motion produced by this small power far exceeded most sanguine expectation.

Two mice killed.

I also killed two mice, one by dividing the vertebræ the neck, the other by confining it under a bell glass co taining about a pint of atmospheric air. The first mo was powerfully excited by a pile of 60 pairs of zinc a copper plates moistened with solution of muriate of so

The one by suffocation not excited.

But the same pile produced so small a degree of motion the second mouse, that I can scarce say whether it mo or not.

Two frogs galvanized in water

After having performed a variety of experiments bef a numerous company with four troughs of 106 pairs plates, containing 5360 inches surface, I placed the po tive and negative wires in a glass jar of water, in whi were two large frogs. The instant both the wires touch the water, the frogs betrayed the greatest signs of unea ness, so much that some gentlemen requested me to remo the wires. I complied with their request, but observed, had every reason to believe they would not survive. T

died in two days.

result was, that on the next day (being left in the wate they appeared very languid, and on the second day th were dead.

Perhaps, Sir, I may presume too far, in submitting m theory or opinion on the above experiments; but if I er I am open to conviction, and shall esteem it a particular f vour to be corrected by any of your scientific readers.

I conceiv

I conceive, Sir, that animals possess a certain portion of excitability, which, by the application of various powerful stimuli, such as galvanism and electricity, produce muscular motions both in the living and dead animal; and if too great a portion be applied, it finally exhausts the excitability, and produces death. But the excitability may also be destroyed by depriving the animal of those things, which are calculated to increase or replenish it: I farther conceive, that the excitability is in proportion to the oxigen the animal or parts of an animal may possess; and if animals are deprived of life by the above means, I am inclined to think little or no motion can be produced by the most powerful stimuli with which we are acquainted. This theory, Sir, I think will account for the results of the experiments I have detailed.

Accounted for on the principle of exhausted irritability.

Thus with respect to the two frogs killed, one by electricity, the other by depriving it of oxigen; the excitability being destroyed in both, it could not be exerted. But the excitability, I conceive, was in some measure restored in the first by its absorbing oxigen from the oximuriatic acid or the atmosphere, after being a few hours exposed to their action. The first mouse, being suddenly deprived of life, still possessed a greater quantity of oxigen than the second, which was killed by depriving it of the vital principle by degrees; and thus it was easily excited, while the latter was not.

The two frogs killed by galvanism may be accounted for on the same principle as the frog killed by electricity.

As this is committed to paper in a hurry, I flatter myself the candid reader will draw a veil over any imperfections. Permit me, Dear Sir, to subscribe myself,

Yours truly,

JOHN TATUM.

53, *Dorset Street*,
July 21, 1808.

Remark.

These experiments appear by no means sufficient to prove, that death is caused by the deprivation of a peculiar principle of excitability, according to the ingenious theory of Brown;

Brown; still less, that oxigen is that principle, an hypothesis I believe first broached by Girtanner. Much indeed must be done, before we can venture to establish any theory on so abstruse a subject, as that of vitality still remains: a subject on which it is not of so much importance to multiply facts, as to describe those that present themselves with accuracy, and with attention to every concomitant circumstance even of the minutest kind. C.

XIII.

On the Structure and Uses of the Spleen. By EVERARD HOME, *Esq. F. R. S.**

Communication between the stomach and circulation through the spleen.

IN bringing forward a fact of so much importance, as a communication between the cardiac portion of the stomach and the circulation of the blood, through the medium of the spleen, I shall not take up the time of the Society by offering any preliminary observations, but state the circumstances which led to the discovery, and the experiments by which the different facts have been ascertained.

Stomach during digestion separated into two portions.

During the investigation of the functions of the stomach, (in which I have been lately engaged,) it was found, that, while digestion is going on, there is a separation between the cardiac and pyloric portions, either by means of a permanent or muscular contraction †. This fact placed the process of digestion in a new light, and led me to consider in what way the quantities of different liquors, which are so often taken into the stomach, can be prevented from being mixed with the half digested food, and interfering with the formation of chyle.

Fluids chiefly contained in the cardiac portion, and carried out of the stomach without reaching the pylorus.

Pursuing this inquiry, I found, that the fluids are principally contained in the cardiac portion, and the food that has

* Philos. Trans. for 1807, p. 45. The president and council of the Royal Society adjudged the medal on Sir Godfrey Copley's donation, for the year 1807, to Mr. Home, for his various papers on anatomy and physiology, printed in the Philosophical Transactions.

† See our Journal, p. 15 of the present vol.

has reached the pyloric portion is usually of one uniform consistence, so that the fluids, beyond what are necessary for digestion, would appear to be carried out of the stomach, without ever reaching so far as the pylorus. To ascertain the truth of this opinion is the object of the present paper.

Lymphatics appear inadequate to this.

The lymphatic vessels of the stomach are numerous, but they are equally or more so in the other viscera. Many circumstances appeared to render it probable, that the spleen is the route by which liquids are conveyed. The more I considered the subject, new reasons in favour of this opinion crowded on my mind, so as almost to enforce conviction, and made me set about devising various methods, by which its truth or falsehood might be established.

Perhaps by the spleen.

Fluids can escape from the stomach, without passing the pylorus.

The first point to be decided was, whether the liquids received into the stomach do escape in any considerable quantity, when prevented from passing out at the pylorus.

This was ascertained by the following experiment, made October 31, 1807, with the assistance of Mr. Brodie, Mr. W. Brande, and Mr. Clift.

This proved on a dog.

The pylorus of a small dog was secured by a ligature, and a few minutes afterwards five ounces by measure of an infusion of indigo in water, of the temperature of the atmosphere, were injected by the mouth into the stomach. At the end of half an hour the dog became sick, and brought up by vomiting 2 ounces of a nearly colourless fluid. The dog was immediately killed, and the different parts were examined. The pylorus was found completely secured by the ligature, so that nothing could pass in that direction. The pyloric portion of the stomach was found empty and contracted; the cardiac portion contained about two ounces of solid contents, enveloped in a gelatinous substance, and one ounce of water with little or no colour, the indigo being completely separated from it, and spread over the surface of the internal membrane. Of the five ounces of water thrown into the stomach, two were brought up by vomiting, and one only remained; two ounces had therefore escaped in the course of half an hour. As the stomach contained two ounces of solid food at the time the experiment was made, it is reasonable to suppose, that there was also

Not by the lymphatics.

also some liquid in it, and in this case the whole quantity that escaped must have exceeded two ounces. On examining the external covering of the stomach, and along the course of the vasa brevia, where the absorbents usually pass, none were discovered, so that these vessels were not at that time carrying any liquid.

The spleen turgid with an aqueous fluid.

The spleen was turgid, unusually large, and its external surface very irregular; when cut into, small cells were every where met with containing a watery fluid, and occupying a considerable portion of its substance. This appearance, which I had never seen before, made me inquire, if it had been taken notice of by others, and endeavour to ascertain the circumstances, under which it is produced. The following statement contains the information, which I have received on this subject.

Malpighi's notion of the spleen.

Malpighi appears to be the first anatomist, who had any particular knowledge of the structure of the spleen. He describes its capsule, and a network which pervades every part of the substance. He mentions a number of small glands, which are hollow, and surrounded by arterial zones, but he had never been able to trace any venal branches into them. He believed, that there was a cellular structure in the spleen containing red blood, interposed between the arteries and veins; this led him to adopt a theory, that the network was muscular, and by its action propelled the blood, so that there was a systole and diastole in the spleen, as in the heart.

Stukely.

Stukely, in his Gulstonian lecture, has very closely copied Malpighi, without giving any additional information.

Cuvier.

Cuvier, the latest writer on this subject, in his *Leçons d'Anatomie comparèe*, corrects the errour of Malpighi respecting the nature of the network, which he states to be composed of elastic ligament, and says, that there are small corpuscles, the use of which is unknown, and which disappear when the blood vessels are minutely injected.

The corpuscles of Cuvier, or glands of Malpighi, contain a fluid, after drinking largely.

In the course of the present investigation, I have examined the spleen after death, under the ordinary circumstances, and have found the appearances described by CUVIER. I have also examined it frequently immediately after the stomach had received unusual quantities of liquids, and in that state

state have found invariably, that the corpuscles of Cuvier, which were the glands of Malpighi, are distinct cells, containing a fluid, which escapes when the cells are punctured, and renders their membranous coat visible; so that it would appear, that the distension of these cells is connected with the state of the stomach, and therefore only takes place occasionally; and that the elastic capsule, by which the spleen is surrounded, adapts the organ to these changes in its volume.

Farther examination of the spleen.

On examining further into the structure of the spleen, in which I have been materially assisted by Mr. Brodie, the following facts have been ascertained.

Arteries ramify in the coats of the cells.

In the spleen of the bullock, horse, and hog, the cells, when the arteries and veins are injected with coloured size, are seen to have numerous arterial branches ramifying in their coats, but no venal ones, which confirms the statement of Malpighi; and when the cells are empty and contracted, and the blood-vessels filled to a great degree of minuteness, the appearance of cells is entirely lost, as stated by Cuvier.

Intermediate parts but few arteries.

When the cells were in a distended state, their cavities in a great many instances were very distinct, having been laid open in making a section of the spleen. The intermediate parts of the spleen are but sparingly supplied with arterial branches, and the smaller ones do not appear to have any particular distribution.

Veins more numerous than the arteries, and radiate from the cells.

When the veins only are injected, their branches appear more numerous, and larger than those of the arteries, making the whole substance of the spleen of a red colour. They appear to arise from the outside of the cells, going off at right angles to their circumference, like radii. Where the injection has not been very minute, they are seen to arise at so many points of the capsule; but where the injection has got into smaller branches, their number is so much increased, that they appear to form plexuses round the cells.

Splenic vein 5 times as large as the artery.

The trunk of the splenic vein, compared with that of the artery, when both are filled with wax, is found to be in the proportion of five to one in its size. This was ascertained both by an accurate measurement of their diameters, and by

Its effects on the urine,

Half an ounce of tincture of rhubarb, diluted in $1\frac{1}{2}$ ounce of water, taken in the interval between meals, did not pass off by urine in less than an hour, and even then was not in sufficient quantity to be discovered, till the test was applied.

The same quantity was taken immediately before a breakfast consisting of tea. In 17 minutes, half an ounce of urine was voided, which when tested had a light tinge. In 30 minutes another half ounce was made, in which the tinge was stronger; and in 41 minutes a third half ounce was made, in which it was very deep. In an hour and ten minutes 7 ounces were voided, in which the tinge of rhubarb was very weak, and in two hours twelve ounces were voided, in which it was hardly perceptible.

and fæces.

In $6\frac{1}{2}$ hours the rhubarb acted on the bowels, and gave a decided tinge to the fæces; the urine made at the same time had a much stronger tinge, than what was voided at one hour and ten minutes.

Gets into the urine by two different channels.

In this experiment, the rhubarb appeared to have escaped from the cardiac portion of the stomach; and in two hours ceased to pass through that channel; but was afterwards carried into the system from the intestines, and again appeared in the urine.

Experiment repeated with similar results.

This experiment was repeated on another person; the rhubarb was detected in the urine in 20 minutes. In 2 hours the tinge became very faint; in 5 hours it was scarcely perceptible; in seven hours the rhubarb acted on the bowels; and the urine made after that period became again as highly tinged as at first.

Prussiate of potash suggested.

It was suggested by a chemical friend, that the prussiate of potash might be a better substance than rhubarb, for the present experiments, since the solution of one quarter of a grain in two ounces of water becomes of a blue colour on the addition of the acidulous muriate of iron.

Not to be detected in the blood in small quantities.

To determine this point, one quarter of a grain was dissolved in two ounces of serum, but no blue colour was produced by the addition of the test, nor did this effect take place till the quantity of the prussiate was increased to a grain; so that minute quantities of the prussiate of potash, or at least of the prussic acid, may exist in the blood, without being detected by adding solution of iron.

The

The effects of rhubarb on the urine, and the different parts of the blood, having been thus ascertained, a third experiment was made, in which that substance was employed, and I had the assistance of the same gentlemen as in the others.

Experiment with rhubarb

On November 17, 1807, at 35 minutes past 11 o'clock, five drams of a mixture of tincture of rhubarb and water, in the proportion of a dram to an ounce, were injected into the stomach of a dog, the pylorus of which was secured. At 20 minutes past one, two ounces of fluid were brought up by vomiting: ten minutes afterwards, another ounce of the mixture was injected, as were nine drams more at half past four o'clock. The two last portions were retained, and at eight o'clock in the evening the dog was killed.

on a dog.

On examining the parts after death, the pylorus was found to be completely secured; the stomach contained about two ounces of fluid; none of the absorbent vessels passing from its great curvature were in a distended state, so as to be rendered visible. The spleen was turgid as in the former experiment, and the urinary bladder full of urine.

None of the lymphatics of the stomach distended,

but the spleen turgid, and bladder full.

This urine, tested by the alkali, received a deeper tinge of rhubarb than the human urine, after rhubarb had been taken three hours by the mouth, and in other respects resembled it.

Rhubarb in the urine;

When the spleen was cut into, the cells were particularly large and distinct. A portion of it was then macerated in two drams of water for ten minutes in a glass vial. All the parts were exposed to the water, by its being divided in all directions. The water thus impregnated was strained off and tested by the alkali, and immediately the reddish brown colour was produced in the centre, and no where else, but in less than a minute it began to diffuse itself, and extended over the whole.

and in the spleen.

A similar portion of the liver was treated in the same way, and the alkali was added to the strained liquor, but no change took place in it whatever.

None in the liver.

In this experiment the rhubarb was detected in the juices of the spleen as well as in the urine; and as there was no appearance of it in the liver, it could not have arrived there

Could not have passed through the common absorbent system.

 through

through the medium of the common absorbents carrying it into the thoracic duct, and afterwards into the circulation of the blood.

The inquiry to be pursued. The discovery of this fact I consider to be of sufficient importance to be announced to the Society, that, when it is thus made public, I may be at liberty more openly, and on a more extensive scale of experiments, to prosecute the inquiry.

XIV.

On the Purification of Lemon Juice. In a Letter from a Correspondent.

To Mr. NICHOLSON,

SIR,

Purification of lemon juice: A READER of your valuable publication submits to your judgment the following methods of purifying lemon juice, which should you think worthy of a place therein, it will oblige

Yours, &c.

PHILOCHEMICUS.

1st, Take of nitromuriate of tin, (prepared by dissolving by nitromuriate of tin; the metal in a mixture of two parts nitric, and one muriatic acid) one dram, lemon juice one quart; after standing forty eight hours filter through white paper.

or charcoal. 2nd, Take of finely pounded and well burned charcoal one ounce; lemon juice one quart; mix, and after standing twelve hours filter through white paper.

Second method perhaps preferable. The latter method seems preferable, as there is nothing employed, which can in any degree injure the juice, the charcoal being perfectly insoluble.

In the former, perhaps some of the solution of tin may pass the filter, though it is most probable it precipitates along with the mucilage and extractive matter, which are so combined, that one cannot be precipitated without the other; however should any pass, the quantity must be so small, as to render it of little consequence.

SCIENTIFIC

SCIENTIFIC NEWS.

New translation of Pliny's Natural History.

I AM informed by Dr. Forbes of Edinburgh, that he is engaged on a translation of Pliny's Natural History, which is to be accompanied with such notes and illustrations, as may be necessary to elucidate the context, a Life of the Author, and a Preliminary Dissertation on the Origin of Natural History, and on its progress and gradual improvement, from its infancy to its present state of comparative maturity.

He observes, that the thirty-seven books of the Natural History of *Caius Plinius Secundus* may with propriety be regarded as the *Encyclopædia* of antiquity, since its very inquisitive and industrious author has collected all the facts recorded by every Greek and Roman writer previous to his own time, concerning the animal, the vegetable, and the mineral kingdoms, and detailed in a clear and luminous arrangement all that the accumulated experience of past ages had ascertained, relative to the nature of *animals* and *vegetables*, to *meterology*, *astronomy*, *botany*, *medicine*, *chemistry*, &c. Pliny's work may be divided into three parts, *geography*, *natural history*, and *materia medica*. Of his geographical inquiries his strictures on the interior parts of Africa are perhaps the most important. He derived the sources of his information on this subject from the Carthaginians; and from what he has recorded respecting the natives and productions of those regions, it is evident, that the ancients were much better acquainted than the moderns are with this quarter of the globe, which from recent events, and from the consequences likely to arise from a great act of national justice, deservedly excites in this country no small share of public interest. The *materia medica* exclusively occupies fifteen books of the *Historia Naturalis*, and constitutes a very curious and instructive department of the author's investigations. It cannot be denied, that Pliny discovers his ignorance in particular points, and that he has recorded with solemn gravity many absurd

New translation of Pliny's Natural History.

absurd fables and anile stories. But perhaps he might have used the language of Quintus Curtius, "*Equidem plura transcribo quam credo;*" and we know, that he occasionally discovers a proper degree of scepticism on various points, which came under his review, and severely rebukes the vanity and self confidence of the Greek authors, from whom he derived his information. Yet, notwithstanding all the censure to which he is obnoxious on the score of credulity, his eloquent and instructive history will ever be regarded as an imperishable monument of its author's indefatigable industry and Roman spirit. Pliny's Natural History is indeed to be considered an invaluable treasure, more especially on account of its containing an infinite number of excerpts and observations illustrative of the various subjects of which the author treats, extracted from the books of many ancient writers, whose works have perished through the injuries of time. It may therefore appear surprising, that no English translation of this admirable performance has been offered to the public for more than two centuries. It is the present translator's object to supply, to the best of his abilities, this *desideratum* in English literature. One great object, which the translator will keep in view in his notes and illustrations, will be, to accommodate Pliny's descriptions of animals, plants, and minerals to the nomenclature of the *Systema Naturæ Linnæi.* This, he is abundantly aware, will prove by much the most difficult part of his labours; and he despairs of executing it with full satisfaction either to the public or to himself. But as in the present state of natural history a translation of Pliny would not be well received without some account of the *synonyms*, he enters on the task in the hope of being able to contribute in some measure toward its accomplishment. The translation thus enlarged must extend to six or seven volumes in octavo, and will be published either in separate volumes successively, or when the whole shall have been finished, as future circumstances may render it advisable.

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Stratford, Printer, Crown-Court, Temple-Bar.